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BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE: LIVERPOOL, 1923

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THE ELECTRICAL STRUCTURE OF MATTER.

BY

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PRESIDENT OF THE ASSOCIATION.

It was in 1896 that this Association last met in Liverpool, under the presidency of the late Lord Lister, that great pioneer in antiseptic surgery, whose memory is held in affectionate remembrance by all nations. His address, which dealt mainly with the history of the application of antiseptic methods to surgery and its connection with the work of Pasteur, that prince of experimenters, whose birth has been so fittingly celebrated this year, gave us in a sense a completed page of brilliant scientific history. At the same time, in his opening remarks, Lister emphasised the importance of the discovery by Röntgen of a new type of radiation, the X-rays, which we now see marked the beginning of a new and fruitful era in another branch of science.

The visit to your city in 1896 was for me a memorable occasion, for it was here that I first attended a meeting of this Association, and here that I read my first scientific paper. But of much more importance, it was here that I benefited by the opportunity, which these gatherings so amply afford, of meeting for the first time many of the distinguished scientific men of this country and the foreign representatives of science who were the guests of this city on that occasion. The year 1896 has always seemed to me a memorable one for other reasons, for on looking back with some sense of perspective we cannot fail to recognise that the last Liverpool meeting marked the beginning of what has been aptly termed the heroic age of Physical Science. Never before in the history of physics has there been witnessed such a period of intense activity when discoveries of fundamental importance have followed one another with such bewildering rapidity.

The discovery of X-rays by Röntgen had been published to the world in 1895, while the discovery of the radioactivity of uranium

by Becquerel was announced early in 1896. Even the most imaginative of our scientific men could never have dreamed at that time of the extension of our knowledge of the structure of matter that was to develop from these two fundamental discoveries, but in the records of the Liverpool meeting we see the dawning recognition of the possible consequences of the discovery of X-rays, not only in their application to medicine and surgery, but as a new and powerful agent for attacking some of the fundamental problems of physics. The address of Professor J. J. Thomson, President of Section A, was devoted mainly to a discussion of the nature of the X-rays, and the remarkable properties induced in gases by the passage of X-rays through them—the beginning of a new and fruitful branch of study.

In applied physics, too, this year marked the beginning of another advance. In the discussion of a paper which I had the honour to read, on a new magnetic detector of electrical waves, the late Sir William Preece told the meeting of the successful transmission of signals for a few hundred yards by electric waves which had been made in England by a young Italian, G. Marconi. The first public demonstration of signalling for short distances by electric waves had been given by Sir Oliver Lodge at the Oxford Meeting of this Association in 1894. It is startling to recall the rapidity of the development from such small beginnings of the new method of wireless intercommunication over the greatest terrestrial distances. In the last few years this has been followed by the even more rapid growth of the allied subject of radiotelephony as a practical means of broadcasting speech and music to distances only limited by the power of the transmitting station. The rapidity of these technical advances is an illustration of the close interconnection that must exist between pure and applied science if rapid and sure progress is to be made. The electrical engineer has been able to base his technical developments on the solid foundation of Maxwell's electromagnetic theory and its complete verification by the researches of Hertz, and also by the experiments of Sir Oliver Lodge in this University—a verification which was completed long before the practical possibilities of this new method of signalling had been generally recognised. The later advances in radiotelegraphy and radiotelephony have largely depended on the application of the results of fundamental researches on the properties of electrons, as illustrated in the use of the thermionic valve or electron tube which has proved such an invaluable agent both for the transmission and reception of electric waves.

It is of great interest to note that the benefits of this union of pure and applied research have not been one-sided. If the fundamental researches of the workers in pure science supply the foundations on which the applications are surely built, the successful practical application in turn quickens and extends the interest of the investigator in

the fundamental problem, while the development of new methods and appliances required for technical purposes often provides the investigator with means of attacking still more difficult questions. This important reaction between pure and applied science can be illustrated in many branches of knowledge. It is particularly manifest in the industrial development of X-ray radiography for therapeutic and industrial purposes, where the development on a large scale of special X-ray tubes and improved methods of excitation has given the physicist much more efficient tools to carry out his researches on the nature of the rays themselves and on the structure of the atom. In this age no one can draw any sharp line of distinction between the importance of so-called pure and applied research. Both are equally essential to progress, and we cannot but recognise that without flourishing schools of research on fundamental matters in our universities and scientific institutions technical research must tend to wither. Fortunately there is little need to labour this point at the moment, for the importance of a training in pure research has been generally recognised. The Department of Scientific and Industrial Research has made a generous provision of grants to train qualified young men of promise in research methods in our scientific institutions, and has aided special fundamental researches which are clearly beyond the capacity of a laboratory to finance from its own funds. Those who have the responsibility of administering the grants in aid of research both for pure and applied science will need all their wisdom and experience to make a wise allocation of funds to secure the maximum of results for the minimum of expenditure. It is fatally easy to spend much money in a direct frontal attack on some technical problem of importance when the solution may depend on some addition to knowledge which can be gained in some other field of scientific inquiry possibly at a trifling cost. It is not in any sense my purpose to criticise those bodies which administer funds for fostering pure and applied research, but to emphasise how difficult it is to strike the correct balance between the expenditure on pure and applied science in order to achieve the best results in the long run.

It is my intention this evening to refer very briefly to some of the main features of that great advance in knowledge of the nature of electricity and matter which is one of the salient features of the interval since the last meeting of this Association in Liverpool.

In order to view the extensive territory which has been conquered by science in this interval, it is desirable to give a brief summary of the state of knowledge of the constitution of matter at the beginning of this epoch. Ever since its announcement by Dalton the atomic theory has steadily gained ground, and formed the philosophic basis for the explanation of the facts of chemical combination. In the early stages of its application to physics and chemistry it was unnecessary

to have any detailed knowledge of the dimensions or structure of the atom. It was only necessary to assume that the atoms acted as individual units, and to know the relative masses of the atoms of the different elements. In the next stage, for example, in the kinetic theory of gases, it was possible to explain the main properties of gases by supposing that the atoms of the gas acted as minute perfectly elastic spheres. During this period, by the application of a variety of methods, many of which were due to Lord Kelvin, rough estimates had been obtained of the absolute dimensions and mass of the atoms. These brought out the minute size and mass of the atom and the enormous number of atoms necessary to produce a detectable effect in any kind of measurement. From this arose the general idea that the atomic theory must of necessity for ever remain unverifiable by direct experiment, and for this reason it was suggested by one school of thought that the atomic theory should be banished from the teaching of chemistry, and that the law of multiple proportions should be accepted as the ultimate fact of Chemistry.

While the vaguest ideas were held as to the possible structure of atoms, there was a general belief among the more philosophically minded that the atoms of the elements could not be regarded as simple unconnected units. The periodic variations of the properties of the elements brought out by Mendeléef were only explicable if atoms were similar structures in some way constructed of similar material. We shall see that the problem of the constitution of atoms is intimately connected with our conception of the nature of electricity. The wonderful success of the electromagnetic theory had concentrated attention on the medium or ether surrounding the conductor of electricity, and little attention had been paid to the actual carriers of the electric current itself. At the same time the idea was generally gaining ground that an explanation of the results of Faraday's experiments on electrolysis was only possible on the assumption that electricity, like matter, was atomic in nature. The name 'electron' had even been given to this fundamental unit by Johnstone Stoney, and its magnitude roughly estimated, but the full recognition of the significance and importance of this conception belongs to the new epoch.

For the clarifying of these somewhat vague ideas, the proof in 1897 of the independent existence of the electron as a mobile electrified unit, of mass minute compared with that of the lightest atom, was of extraordinary importance. It was soon seen that the electron must be of a constituent of all the atoms of matter, and that optical spectra had their origin in their vibrations. The discovery of the electron and the proof of its liberation by a variety of methods from all the atoms of matter was of the utmost significance, for it strengthened the view that the electron was probably the common unit in the

structure of atoms which the periodic variation of the chemical properties had indicated. It gave for the first time some hope of the success of an attack on that most fundamental of all problems—the detailed structure of the atom. In the early development of this subject science owes much to the work of Sir J. J. Thomson, both for the boldness of his ideas and for his ingenuity in developing methods for estimating the number of electrons in the atom, and of probing its structure. He early took the view that the atom must be an electrical structure, held together by electrical forces, and showed in a general way lines of possible explanation of the variation of physical and chemical properties of the elements, exemplified in the periodic law.

In the meantime our whole conception of the atom and of the magnitude of the forces which held it together were revolutionised by the study of radioactivity. The discovery of radium was a great step in advance, for it provided the experimenter with powerful sources of radiation specially suitable for examining the nature of the characteristic radiations which are emitted by the radioactive bodies in general. It was soon shown that the atoms of radioactive matter were undergoing spontaneous transformation, and that the characteristic radiations emitted, viz. the α, β, and γ rays, were an accompaniment and consequence of these atomic explosions. The wonderful succession of changes that occur in uranium, more than thirty in number, was soon disclosed and simply interpreted on the transformation theory. The radioactive elements provide us for the first time with a glimpse into Nature's laboratory, and allow us to watch and study but not control the changes that have their origin in the heart of the radioactive atoms. These atomic explosions involve energies which are gigantic compared with those involved in any ordinary physical or chemical process. In the majority of cases an α particle is expelled at high speed, but in others a swift electron is ejected often accompanied by a γ ray, which is a very penetrating X-ray of high frequency. The proof that the α particle is a charged helium atom for the first time disclosed the importance of helium as one of the units in the structure of the radioactive atoms, and probably also in that of the atoms of most of the ordinary elements. Not only then have the radioactive elements had the greatest direct influence on natural philosophy, but in subsidiary ways they have provided us with experimental methods of almost equal importance. The use of α particles as projectiles with which to explore the interior of the atom has definitely exhibited its nuclear structure, has led to artificial disintegration of certain light atoms, and promises to yield more information yet as to the actual structure of the nucleus itself.

The influence of radioactivity has also extended to yet another field of study of fascinating interest. We have seen that the first rough

estimates of the size and mass of the atom gave little hope that we could detect the effect of a single atom. The discovery that the radioactive bodies expel actual charged atoms of helium with enormous energy altered this aspect of the problem. The energy associated with a single α particle is so great that it can readily be detected by a variety of methods. Each α particle, as Sir Wm. Crookes first showed, produces a flash of light easily visible in a dark room when it falls on a screen coated with crystals of zinc sulphide. This scintillation method of counting individual particles has proved invaluable in many researches, for it gives us a method of unequalled delicacy for studying the effects of single atoms. The α particle can also be detected electrically or photographically, but the most powerful and beautiful of all methods is that perfected by Mr. C. T. R. Wilson for observing the track through a gas not only of an α particle but of any type of penetrating radiation which produces ions or of electrified particles along its path. The method is comparatively simple, depending on the fact, first discovered by him, that if a gas saturated with moisture is suddenly cooled each of the ions produced by the radiation becomes the nucleus of a visible drop of water. The water-drops along the track of the α particle are clearly visible to the eye, and can be recorded photographically. These beautiful photographs of the effect produced by single atoms or single electrons appeal, I think, greatly to all scientific men. They not only afford convincing evidence of the discrete nature of these particles, but give us new courage and confidence that the scientific methods of experiment and deduction are to be relied upon in this field of inquiry; for many of the essential points brought out so clearly and concretely in these photographs were correctly deduced long before such confirmatory photographs were available. At the same time, a minute study of the detail disclosed in these photographs gives us most valuable information and new clues on many recondite effects produced by the passage through matter of these flying projectiles and penetrating radiations.

In the meantime a number of new methods had been devised to fix with some accuracy the mass of the individual atom and the number in any given quantity of matter. The concordant results obtained by widely different physical principles gave great confidence in the correctness of the atomic idea of matter. The method found capable of most accuracy depends on the definite proof of the atomic nature of electricity and the exact valuation of this fundamental unit of charge. We have seen that it was early surmised that electricity was atomic in nature. This view was confirmed and extended by a study of the charges carried by electrons, α particles, and the ions produced in gases by X-rays and the rays from radioactive matter. It was first shown by Townsend that the positive or negative charge carried by an ion in

gases was invariably equal to the charge carried by the hydrogen ion in the electrolysis of water, which we have seen was assumed, and assumed correctly, by Johnstone Stoney to be the fundamental unit of charge. Various methods were devised to measure the magnitude of this fundamental unit; the best known and most accurate is Millikan's, which depends on comparing the pull of an electric field on a charged droplet of oil or mercury with the weight of the drop. His experiments gave a most convincing proof of the correctness of the electronic theory, and gave a measure of this unit, the most fundamental of all physical units, with an accuracy of about one in a thousand. Knowing this value, we can by the aid of electrochemical data easily deduce the mass of the individual atoms and the number of molecules in a cubic centimetre of any gas with an accuracy of possibly one in a thousand, but certainly better than one in a hundred. When we consider the minuteness of the unit of electricity and of the mass of the atom this experimental achievement is one of the most notable even in an era of great advances.

The idea of the atomic nature of electricity is very closely connected with the attack on the problem of the structure of the atom. If the atom is an electrical structure it can only contain an integral number of charged units, and, since it is ordinarily neutral, the number of units of positive charge must equal the number of negative. One of the main difficulties in this problem has been the uncertainty as to the relative part played by positive and negative electricity in the structure of the atom. We know that the electron has a negative charge of one fundamental unit, while the charged hydrogen atom, whether in electrolysis or in the electric discharge, has a charge of one positive unit. But the mass of the electron is only 1/1840 of the mass of the hydrogen atom, and though an extensive search has been made, not the slightest evidence has been found of the existence of a positive electron of small mass like the negative. In no case has a positive charge been found associated with a mass less than that of the charged atom of hydrogen. This difference between positive and negative electricity is at first sight very surprising, but the deeper we pursue our inquiries the more this fundamental difference between the units of positive and negative electricity is emphasised. In fact, as we shall see later, the atoms are quite unsymmetrical structures with regard to the positive and negative units contained in them, and indeed it seems certain that if there were not this difference in mass between the two units, matter, as we know it, could not exist.

It is natural to inquire what explanation can be given of this striking difference in mass of the two units. I think all scientific men are convinced that the small mass of the negative electron is to be entirely associated with the energy of its electrical structure, so that the electron

may be regarded as a disembodied atom of negative electricity. We know that an electron in motion, in addition to possessing an electric field, also generates a magnetic field around it, and energy in the electromagnetic form is stored in the medium and moves with it. This gives the electron an apparent or electrical mass, which, while nearly constant for slow speeds, increases rapidly as its velocity approaches that of light. This increase of mass is in good accord with calculation, whether based on the ordinary electrical theory or on the theory of relativity. Now we know that the hydrogen atom is the lightest of all atoms, and is presumably the simplest in structure, and that the charged hydrogen atom, which we shall see is to be regarded as the hydrogen nucleus, carries a unit positive charge. It is thus natural to suppose that the hydrogen nucleus is the atom of positive electricity, or positive electron, analogous to the negative electron, but differing from it in mass. Electrical theory shows that the mass of a given charge of electricity increases with the concentration, and the greater mass of the hydrogen nucleus would be accounted for if its size were much smaller than that of the electron. Such a conclusion is supported by evidence obtained from the study of the close collisions of α particles with hydrogen nuclei. It is found that the hydrogen nucleus must be of minute size, of radius less than the electron, which is usually supposed to be about 10^{-13} cms.; also the experimental evidence is not inconsistent with the view that the hydrogen nucleus may actually be much smaller than the electron. While the greater mass of the positive atom of electricity may be explained in this way, we are still left with the enigma why the two units of electricity should differ so markedly in this respect. In the present state of our knowledge it does not seem possible to push this inquiry further, or to discuss the problem of the relation of these two units.

We shall see that there is the strongest evidence that the atoms of matter are built up of these two electrical units, viz. the electron and the hydrogen nucleus or proton, as it is usually called when it forms part of the structure of any atom. It is probable that these two are the fundamental and indivisible units which build up our universe, but we may reserve in our mind the possibility that further inquiry may some day show that these units are complex, and divisible into even more fundamental entities. On the views we have outlined the mass of the atom is the sum of the electrical masses of the individual charged units composing its structure, and there is no need to assume that any other kind of mass exists. At the same time, it is to be borne in mind that the actual mass of an atom may be somewhat less than the sum of the masses of component positive and negative electrons when in the free state. On account of the very close proximity of the charged units in the nucleus of an atom, and the consequent disturbance

of the electric and magnetic field surrounding them, such a decrease of mass is to be anticipated on general theoretical grounds.

We must now look back again to the earlier stages of the present epoch in order to trace the development of our ideas on the detailed structure of the atom. That electrons as such were important constituents was clear by 1900, but little real progress followed until the part played by the positive charges was made clear. New light was thrown on this subject by examining the deviation of α particles when they passed through the atoms of matter. It was found that occasionally a swift α particle was deflected from its rectilinear path through more than a right angle by an encounter with a single atom. In such a collision the laws of dynamics ordinarily apply, and the relation between the velocities of the colliding atoms before and after collision are exactly the same as if the two colliding particles are regarded as perfectly elastic spheres of minute dimensions. It must, however, be borne in mind that in these atomic collisions there is no question of mechanical impacts such as we observe with ordinary matter. The reaction between the two particles occurs through the intermediary of the powerful electric fields that surround them. Beautiful photographs illustrating the accuracy of these laws of collision between an α particle and an atom have been obtained by Messrs. Wilson, Blackett, and others, while Mr. Wilson has recently obtained many striking illustrations of collisions between two electrons. Remembering the great kinetic energy of the α particle, its deflection through a large angle in a single atomic encounter shows clearly that very intense deflecting forces exist inside the atom. It seemed clear that electric fields of the required magnitude could be obtained only if the main charge of the atom were concentrated in a minute nucleus. From this arose the conception of the nuclear atom, now so well known, in which the heart of the atom is supposed to consist of a minute but massive nucleus, carrying a positive charge of electricity, and surrounded at a distance by the requisite number of electrons to form a neutral atom.

A detailed study of the scattering of α particles at different angles, by Geiger and Marsden, showed that the results were in close accord with this theory, and that the intense electric forces near the nucleus varied according to the ordinary inverse square law. In addition, the experiments allowed us to fix an upper limit for the dimensions of the nucleus. For a heavy atom like that of gold the radius of the nucleus, if supposed to be spherical, was less than one thousandth of the radius of the complete atom surrounded by its electrons, and certainly less than 4×10^{-12} cms. All the atoms were found to show this nuclear structure, and an approximate estimate was made of the nuclear charge of different atoms. This type of nuclear atom, based on direct experimental evidence, possesses some very simple properties. It is obvious

that the number of units of resultant positive charge in the nucleus fixes the number of the outer planetary electrons in the neutral atom. In addition, since these outer electrons are in some way held in equilibrium by the attractive forces from the nucleus, and, since we are confident from general physical and chemical evidence that all atoms of any one element are identical in their external structure, it is clear that their arrangement and motion must be governed entirely by the magnitude of the nuclear charge. Since the ordinary chemical and physical properties are to be ascribed mainly to the configuration and motion of the outer electrons, it follows that the properties of an atom are defined by a whole number representing its nuclear charge. It thus becomes of great importance to determine the value of this nuclear charge for the atoms of all the elements.

Data obtained from the scattering of α particles, and also from the scattering of X-rays by light elements, indicated that the nuclear charge of an element was numerically equal to about half the atomic weight in terms of hydrogen. It was fairly clear from general evidence that the hydrogen nucleus had a charge one, and the helium nucleus (the α particle) a charge two. At this stage another discovery of great importance provided a powerful method of attack on this problem. The investigation by Laue on the diffraction of X-rays by crystals had shown definitely that X-rays were electromagnetic waves of much shorter wave-length than light, and the experiments of Sir William Bragg and W. L. Bragg had provided simple methods for studying the spectra of a beam of X-rays. It was found that the spectrum in general shows a continuous background on which is superimposed a spectrum of bright lines. At this stage H. G. J. Moseley began a research with the intention of deciding whether the properties of an element depended on its nuclear charge rather than on its atomic weight as ordinarily supposed. For this purpose the X-ray spectra emitted by a number of elements were examined and found to be all similar in type. The frequency of a given line was found to vary very nearly as the square of a whole number which varied by unity in passing from one element to the next. Moseley identified this whole number with the atomic or ordinal number of the elements when arranged in increasing order of atomic weight, allowance being made for the known anomalies in the periodic table and for certain gaps corresponding to possible but missing elements. He concluded that the atomic number of an element was a measure of its nuclear charge, and the correctness of this deduction has been recently verified by Chadwick by direct experiments on the scattering of α particles. Moseley's discovery is of fundamental importance, for it not only fixes the number of electrons in all the atoms, but shows conclusively that the properties of an atom, as had been surmised, are determined not by its atomic weight but

by its nuclear charge. A relation of unexpected simplicity is thus found to hold between the elements. No one could have anticipated that with few exceptions all atomic numbers between hydrogen 1, and uranium 92, would correspond to known elements. The great power of Moseley's law in fixing the atomic number of an element is well illustrated by the recent discovery by Coster and Hevesy in Copenhagen of the missing element of atomic number 72, which they have named 'hafnium.'

Once the salient features of the structure of atoms have been fixed and the number of electrons known, the further study of the structure of the atom falls naturally into two great divisions: one, the arrangement of the outer electrons which controls the main physical and chemical properties of an element, and the other the structure of the nucleus on which the mass and radioactivity of the atom depends. On the nuclear theory the hydrogen atom is of extreme simplicity, consisting of a singly-charged positive nucleus with only one attendant electron. The position and motions of the single electron must account for the complicated optical spectrum, and whatever physical and chemical properties are to be attributed to the hydrogen atom. The first definite attack on the problem of the electronic structure of the atom was made by Niels Bohr. He saw clearly that, if this simple constitution was assumed, it is impossible to account for the spectrum of hydrogen on the classical electrical theories, but that a radical departure from existing views was necessary. For this purpose he applied to the atom the essential ideas of the Quantum Theory which had been developed by Planck for other purposes, and had been found of great service in explaining many fundamental difficulties in other branches of science. On Planck's theory radiation is emitted in definite units or quanta, in which the energy E of a radiation is equal to $h\nu$ where ν is the frequency of the radiation measured by the ordinary methods and h a universal constant. This quantum of radiation is not a definite fixed unit like the atom of electricity, for its magnitude depends on the frequency of the radiation. For example, the energy of a quantum is small for visible light, but becomes large for radiation of high frequency corresponding to the X-rays or the γ rays from radium.

Time does not allow me to discuss the underlying meaning of the quantum theory or the difficulties connected with it. Certain aspects of the difficulties were discussed in the Presidential Address before this Association by Sir Oliver Lodge at Birmingham in 1913. It suffices to say that this theory has proved of great value in several branches of science, and is supported by a large mass of direct experimental evidence.

In applying the quantum theory to the structure of the hydrogen

atom Bohr supposed that the single electron could move in a number of stable orbits, controlled by the attractive force of the nucleus, without losing energy by radiation. The position and character of these orbits were defined by certain quantum relations depending on one or more whole numbers. It was assumed that radiation was only emitted when the electron for some reason was transferred from one stable orbit to another of lower energy. In such a case it was supposed that a homogeneous radiation was emitted of frequency ν determined by the quantum relation $E=h\nu$ where E was the difference of the energy of the electron in the two orbits. Some of these possible orbits are circular, others elliptical, with the nucleus as a focus, while if the change of mass of the electron with velocity is taken into account the orbits, as Sommerfeld showed, depend on two quantum numbers, and are not closed, but consist of a nearly elliptical orbit slowly rotating round the nucleus. In this way it is possible not only to account for the series relations between the bright lines of the hydrogen spectrum, but also to explain the fine structure of the lines and the very complicated changes observed when the radiating atoms are exposed in a strong magnetic or electric field. Under ordinary conditions the electron in the hydrogen atom rotates in a circular orbit close to the nucleus, but if the atoms are excited by an electric discharge or other suitable method, the electron may be displaced and occupy any one of the stable positions specified by the theory. In a radiating gas giving the complete hydrogen spectrum there will be present many different kinds of hydrogen atoms, in each of which the electron describes one of the possible orbits specified by the theory. On this view it is seen that the variety of modes of vibration of the hydrogen atom is ascribed, not to complexity of the structure of the atom, but to the variety of stable orbits which an electron may occupy relative to the nucleus. This novel theory of the origin of spectra has been developed so as to apply not only to hydrogen but to all the elements, and has been instrumental in throwing a flood of light on the relations and origin of their spectra, both X-ray and optical. The information thus gained has been applied by Bohr to determine the distribution of the electrons round the nucleus of any atom. The problem is obviously much less complicated for hydrogen than for a heavy atom, where each of the large number of electrons present acts on the other, and where the orbits described are much more intricate than the orbit of the single electron in hydrogen. Notwithstanding the great difficulties of such a complicated system of electrons in motion, it has been possible to fix the quantum numbers that characterise the motion of each electron, and to form at any rate a rough idea of the character of the orbit.

These planetary electrons divide themselves up into groups, accord-

ing as their orbits are characterised by one or more equal quantum numbers. Without going into detail a few examples may be given to illustrate the conclusions which have been reached. As we have seen, the first element hydrogen has a nuclear charge of 1 and 1 electron; the second, helium, has a charge 2 and 2 electrons, moving in coupled orbits on the detailed nature of which there is still some uncertainty. These two electrons form a definite group, known as the K group, which is common to all the elements except hydrogen. For increasing nuclear charge the K group of electrons retain their characteristics, but move with increasing speed, and approach closer to the nucleus. As we pass from helium of atomic number 2 to neon, number 10, a new group of electrons is added consisting of two sub-groups, each of four electrons, together called the L group. This L group appears in all atoms of higher atomic number, and, as in the case of the K group, the speed of motion of the electrons increases, and the size of their orbits diminishes with the atomic number. When once the L group has been completed a new and still more complicated M group of electrons begins forming outside it, and a similar process goes on until uranium, which has the highest atomic number, is reached.

It may be of interest to try to visualise the conception of the atom we have so far reached by taking for illustration the heaviest atom, uranium. At the centre of the atom is a minute nucleus surrounded by a swirling group of 92 electrons, all in motion in definite orbits, and occupying but by no means filling a volume very large compared with that of the nucleus. Some of the electrons describe nearly circular orbits round the nucleus; others, orbits of a more elliptical shape whose axes rotate rapidly round the nucleus. The motion of the electrons in the different groups is not necessarily confined to a definite region of the atom, but the electrons of one group may penetrate deeply into the region mainly occupied by another group, thus giving a type of inter-connection or coupling between the various groups. The maximum speed of any electron depends on the closeness of the approach to the nucleus, but the outermost electron will have a minimum speed of more than 1,000 kilometres per second, while the innermost K electrons have an average speed of more than 150,000 kilometres per second, or half the speed of light. When we visualise the extraordinary complexity of the electronic system we may be surprised that it has been possible to find any order in the apparent medley of motions.

In reaching these conclusions, which we owe largely to Professor Bohr and his co-workers, every available kind of data about the different atoms has been taken into consideration. A study of the X-ray spectra, in particular, affords information of great value as to the arrangement of the various groups in the atom, while the optical spectrum and general chemical properties are of great importance in deciding the

arrangements of the superficial electrons. While the solution of the grouping of the electrons proposed by Bohr has been assisted by considerations of this kind, it is not empirical in character, but has been largely based on general theoretical considerations of the orbits of electrons that are physically possible on the generalised quantum theory. The real problem involved may be illustrated in the following way. Suppose the gold nucleus be in some way stripped of its attendant seventy-nine electrons and that the atom is reconstituted by the successive addition of electrons one by one. According to Bohr, the atom will be reorganised in one way only, and one group after another will successively form and be filled up in the manner outlined. The nucleus atom has often been likened to a solar system where the sun corresponds to the nucleus and the planets to the electrons. The analogy, however, must not be pressed too far. Suppose, for example, we imagined that some large and swift celestial visitor traverses and escapes from our solar system without any catastrophe to itself or the planets. There will inevitably result permanent changes in the lengths of the month and year, and our system will never return to its original state. Contrast this with the effect of shooting an electron or α particle through the electronic structure of the atom. The motion of many of the electrons will be disturbed by its passage, and in special cases an electron may be removed from its orbit and hurled out of its atomic system. In a short time another electron will fall into the vacant place from one of the outer groups, and this vacant place in turn will be filled up, and so on until the atom is again reorganised. In all cases the final state of the electronic system is the same as in the beginning. This illustration also serves to indicate the origin of the X-rays excited in the atom, for these arise in the process of reformation of an atom from which an electron has been ejected, and the radiation of highest frequency arises when the electron is removed from the K group.

It is possibly too soon to express a final opinion on the accuracy of this theory which defines the outer structure of the atom, but there can be no doubt that it constitutes a great advance. Not only does it offer a general explanation of the optical and X-ray spectra of the atom, but it accounts in detail for many of the most characteristic features of the periodic law of Mendeléef. It gives us for the first time a clear idea of the reason for the appearance in the family of elements of groups of consecutive elements with similar chemical properties, such as the groups analogous to the iron group and the unique group of rare earths. The theory of Bohr, like all living theories, has not only correlated a multitude of isolated facts known about the atom, but has shown its power to predict new relations which can be verified by experiment. For example, the theory predicted the relations which must subsist between the Rydberg constants of the arc and spark spectra, and generally

between all the successive optical spectra of an element, a prediction so strikingly confirmed by Paschen's work on the spectrum of doubly ionized aluminium and Fowler's work on the spectrum of trebly ionized silicon. Finally, it predicted with such great confidence the chemical properties of the missing element, number 72, that it gave the necessary incentive for its recent discovery.

While the progress of our knowledge of the outer structure of atoms has been much more rapid than could have been anticipated, we clearly see that only a beginning has been made on this great problem, and that an enormous amount of work is still required before we can hope to form anything like a complete picture even of the outer structure of the atom. We may be confident that the main features of the structure are clear, but in a problem of such great complexity progress in detail must of necessity be difficult and slow.

We have not so far referred to the very difficult question of the explanation on this theory of the chemical combination of atoms. In fact, as yet the theory has hardly concerned itself with molecular structure. On the chemical side, however, certain advances have already been made, notably by G. N. Lewis, Kossel, and Langmuir, in the interpretation of the chemical evidence by the idea of shared electrons, which play a part in the electronic structure of two combined atoms. There can be little doubt that the next decade will see an intensified attack by physicists and chemists on this very important but undoubtedly very complicated question.

Before leaving this subject, it may be of interest to refer to certain points in Bohr's theory of a more philosophical nature. It is seen that the orbits and energies of the various groups of electrons can be specified by certain quantum numbers, and the nature of the radiation associated with a change of orbit can be defined. But at the same time we cannot explain why these orbits are alone permissible under normal conditions, or understand the mechanism by which radiation is emitted. It may be quite possible to formulate accurately the energy relation of the electrons in the atom on a simple theory, and to explain in considerable detail all the properties of an atom, without any clear understanding of the underlying processes which lead to these results. It is natural to hope that with advance of knowledge we may be able to grasp the details of the process which leads to the emission of radiation, and to understand why the orbits of the electrons in the atom are defined by the quantum relations. Some, however, are inclined to take the view that in the present state of knowledge it may be quite impossible in the nature of things to form that detailed picture in space and time of successive events that we have been accustomed to consider as so important a part of a complete theory. The atom is naturally the most fundamental structure presented to us. Its properties must explain the properties

of all more complicated structures, including matter in bulk, but we may not, therefore, be justified in expecting that its processes can be explained in terms of concepts derived entirely from a study of molar properties. The atomic processes involved may be so fundamental that a complete understanding may be denied us. It is early yet to be pessimistic on this question, for we may hope that our difficulties may any day be resolved by further discoveries.

We must now turn our attention to that new and comparatively unexplored territory, the nucleus of the atom. In a discussion on the structure of the atom ten years ago, in answer to a question on the structure of the nucleus, I was rash enough to say that it was a problem that might well be left to the next generation, for at that time there seemed to be few obvious methods of attack to throw light on its constitution. While much more progress has been made than appeared possible at that time, the problem of the structure of the nucleus is inherently more difficult than the allied problem already considered of the structure of the outer atom, where we have a wealth of information obtained from the study of light and X-ray spectra and from the chemical properties to test the accuracy of our theories.

In the case of the nucleus, we know its resultant charge, fixed by Moseley's law, and its mass, which is very nearly equal to the mass of the whole atom, since the mass of the planetary electrons is relatively very small and may for most purposes be neglected. We know that the nucleus is of size minute compared with that of the whole atom, and can with some confidence set a maximum limit to its size. The study of radioactive bodies has provided us with very valuable information on the structure of the nucleus, for we know that the α and β particles must be expelled from it, and there is strong evidence that the very penetrating γ rays represent modes of vibration of the electrons contained in its structure. In the long series of transformations which occur in the uranium atom, eight α particles are emitted and six electrons, and it seems clear that the nucleus of a heavy atom is built up, in part at least, of helium nuclei and electrons. It is natural to suppose that many of the ordinary stable atoms are constituted in a similar way. It is a matter of remark that no indication has been obtained that the lightest nucleus, viz. that of hydrogen, is liberated in these transformations, where the processes occurring are of so fundamental a character. At the same time, it is evident that the hydrogen nucleus must be a unit in the structure of some atoms, and this has been confirmed by direct experiment. Dr. Chadwick and I have observed that swift hydrogen nuclei are released from the elements boron, nitrogen, fluorine, sodium, aluminium, and phosphorus when they are bombarded by swift α particles, and there is little room for doubt that these hydrogen nuclei form an essential part of the nuclear structure.

The speed of ejection of these nuclei depends on the velocity of the α particle and on the element bombarded. It is of interest to note that the hydrogen nuclei are liberated in all directions, but the speed in the backward direction is always somewhat less than in the direction of the α particle. Such a result receives a simple explanation if we suppose that the hydrogen nuclei are not built into the main nucleus but exist as satellites probably in motion round a central core. There can be no doubt that bombardment by α particles has effected a veritable disintegration of the nuclei of this group of elements. It is significant that the liberation of hydrogen nuclei only occurs in elements of odd atomic number, viz. 5, 7, 9, 11, 13, 15, the elements of even number appearing quite unaffected. For a collision of an α particle to be effective, it must either pass close to the nucleus or actually penetrate its structure. The chance of this is excessively small on account of the minute size of the nucleus. For example, although each individual α particle will pass through the outer structure of more than 100,000 atoms of aluminium in its path, it is only about one α particle in a million that gets close enough to the nucleus to effect the liberation of its hydrogen satellite.

This artificial disintegration of elements by α particles takes place only on a minute scale, and its observation has only been possible by the counting of individual swift hydrogen nuclei by the scintillations they produce in zinc sulphide.

These experiments suggest that the hydrogen nucleus or proton must be one of the fundamental units which build up a nucleus, and it seems highly probable that the helium nucleus is a secondary building unit composed of the very close union of four protons and two electrons. The view that the nuclei of all atoms are ultimately built up of protons of mass nearly one and of electrons has been strongly supported and extended by the study of *isotopes*. It was early observed that some of the radioactive elements which showed distinct radioactive properties were chemically so alike that it was impossible to effect their separation when mixed together. Similar elements of this kind were called 'isotopes' by Soddy, since they appeared to occupy the same place in the periodic table. For example, a number of radioactive elements in the uranium and thorium series have been found to have physical and chemical properties identical with those of ordinary lead, but yet to have atomic weights differing from ordinary lead, and also distinctive radioactive properties. The nuclear theory of the atom offers at once a simple interpretation of the relation between isotopic elements. Since the chemical properties of an element are controlled by its nuclear charge and little influenced by its mass, isotopes must correspond to atoms with the same nuclear charge but of different nuclear mass. Such a view also offers a simple explanation why the radioactive isotopes show

different radioactive properties, for it is to be anticipated that the stability of a nucleus will be much influenced by its mass and arrangement.

Our knowledge of isotopes has been widely extended in the last few years by Aston, who has devised an accurate direct method for showing the presence of isotopes in the ordinary elements. He has found that some of the elements are 'pure'—*i.e.* consist of atoms of identical mass—while others contain a mixture of two or more isotopes. In the case of the isotopic elements, the atomic mass, as ordinarily measured by the chemist, is a mean value depending on the atomic masses of the individual isotopes and their relative abundance. These investigations have not only shown clearly that the number of distinct species of atoms is much greater than was supposed, but have brought out a relation between the elements of great interest and importance. The atomic masses of the isotopes of most of the elements examined have been found, to an accuracy of about one in a thousand, to be whole numbers in terms of oxygen, 16. This indicates that the nuclei are ultimately built up of protons of mass very nearly one and of electrons. It is natural to suppose that this building unit is the hydrogen nucleus, but that its average mass in the complex nucleus is somewhat less than its mass in the free state owing to the close packing of the charged units in the nuclear structure. We have already seen that the helium nucleus of mass 4 is probably a secondary unit of great importance in the building up of many atoms, and it may be that other simple combinations of protons and electrons of mass 2 and 3 occur in the nucleus, but these have not been observed in the free state.

While the mass of the majority of the isotopes are nearly whole numbers, certain cases have been observed by Aston where this rule is slightly departed from. Such variations in mass may ultimately prove of great importance in throwing light on the arrangement and closeness of packing of the protons and electrons, and for this reason it is to be hoped that it may soon prove possible to compare atomic masses of the elements with much greater precision even than at present.

While we may be confident that the proton and the electron are the ultimate units which take part in the building up of all nuclei, and can deduce with some certainty the number of protons and electrons in the nuclei of all atoms, we have little, if any, information on the distribution of these units in the atom or on the nature of the forces that hold them in equilibrium. While it is known that the law of the inverse square holds for the electrical forces some distance from the nucleus, it seems certain that this law breaks down inside the nucleus. A detailed study of the collisions between α particles and hydrogen atoms, where the nuclei approach very close to each other, shows that the forces between nuclei increase ultimately much more rapidly than is to be expected from the law of the inverse square, and it may be that new and unex-

pected forces may come into importance at the very small distances separating the protons and electrons in the nucleus. Until we gain more information on the nature and law of variation of the forces inside the nucleus, further progress on the detailed structure of the nucleus may be difficult. At the same time, there are still a number of hopeful directions in which an attack may be made on this most difficult of problems. A detailed study of the γ rays from radioactive bodies may be expected to yield information as to the motion of the electrons inside the nucleus, and it may be, as Ellis has suggested, that quantum laws are operative inside as well as outside the nucleus. From a study of the relative proportions of the elements in the earth's crust, Harkins has shown that elements of even atomic number are much more abundant than elements of odd number, suggesting a marked difference of stability in these two classes of elements. It seems probable that any process of stellar evolution must be intimately connected with the building up of complex nuclei from simpler ones, and its study may thus be expected to throw much light on the evolution of the elements.

The nucleus of a heavy atom is undoubtedly a very complicated system, and in a sense a world of its own, little, if at all, influenced by the ordinary physical and chemical agencies at our command. When we consider the mass of a nucleus compared with its volume it seems certain that its density is many billions of times that of our heaviest element. Yet, if we could form a magnified picture of the nucleus, we should expect that it would show a discontinuous structure, occupied but not filled by the minute building units, the protons and electrons, in ceaseless rapid motion controlled by their mutual forces.

Before leaving this subject it is desirable to say a few words on the important question of the energy relations involved in the formation and disintegration of atomic nuclei, first opened up by the study of radioactivity. For example, it is well known that the total evolution of energy during the complete disintegration of one gramme of radium is many millions of times greater than in the complete combustion of an equal weight of coal. It is known that this energy is initially mostly emitted in the kinetic form of swift α and β particles, and the energy of motion of these bodies is ultimately converted into heat when they are stopped by matter. Since it is believed that the radioactive elements were analogous in structure to the ordinary inactive elements the idea naturally arose that the atoms of all the elements contained a similar concentration of energy, which would be available for use if only some simple method could be discovered of promoting and controlling their disintegration. This possibility of obtaining new and cheap sources of energy for practical purposes was naturally an alluring prospect to the lay and scientific man alike. It is quite true

that, if we were able to hasten the radioactive processes in uranium and thorium so that the whole cycle of their disintegration could be confined to a few days instead of being spread over thousands of millions of years, these elements would provide very convenient sources of energy on a sufficient scale to be of considerable practical importance. Unfortunately, although many experiments have been tried, there is no evidence that the rate of disintegration of these elements can be altered in the slightest degree by the most powerful laboratory agencies. With increase in our knowledge of atomic structure there has been a gradual change of our point of view on this important question, and there is by no means the same certainty to-day as a decade ago that the atoms of an element contain hidden stores of energy. It may be worth while to spend a few minutes in discussing the reason for this change in outlook. This can best be illustrated by considering an interesting analogy between the transformation of a radioactive nucleus and the changes in the electron arrangement of an ordinary atom. It is now well known that it is possible by means of electron bombardment or by appropriate radiation to excite an atom in such a way that one of its superficial electrons is displaced from its ordinary stable position to another temporarily stable position further removed from the nucleus. This electron in course of time falls back into its old position, and its potential energy is converted into radiation in the process. There is some reason for believing that the electron has a definite average life in the displaced position, and that the chance of its return to its original position is governed by the laws of probability. In some respects an 'excited' atom of this kind is thus analogous to a radioactive atom, but of course the energy released in the disintegration of a nucleus is of an entirely different order of magnitude from the energy released by return of the electron in the excited atom. It may be that the elements, uranium and thorium, represent the sole survivals in the earth to-day of types of elements that were common in the long distant ages, when the atoms now composing the earth were in course of formation. A fraction of the atoms of uranium and thorium formed at that time has survived over the long interval on account of their very slow rate of transformation. It is thus possible to regard these atoms as having not yet completed the cycle of changes which the ordinary atoms have long since passed through, and that the atoms are still in the 'excited' state where the nuclear units have not yet arranged themselves in positions of ultimate equilibrium, but still have a surplus of energy which can only be released in the form of the characteristic radiation from active matter. On such a view, the presence of a store of energy ready for release is not a property of all atoms, but only of a special class of atoms like the radioactive atoms which have not yet reached the final state for equilibrium.

It may be urged that the artificial disintegration of certain elements by bombardment with swift α particles gives definite evidence of a store of energy in some of the ordinary elements, for it is known that a few of the hydrogen nuclei, released from aluminium for example, are expelled with such swiftness that the particle has a greater individual energy than the α particle which causes their liberation. Unfortunately, it is very difficult to give a definite answer on this point until we know more of the details of this disintegration.

On the other hand, another method of attack on this question has become important during the last few years, based on the comparison of the relative masses of the elements. This new point of view can best be illustrated by a comparison of the atomic masses of hydrogen and helium. As we have seen, it seems very probable that helium is not an ultimate unit in the structure of nuclei, but is a very close combination of four hydrogen nuclei and two electrons. The mass of the helium nucleus, 4.00 in terms of $O=16$, is considerably less than the mass 4.03 of four hydrogen nuclei. On modern views there is believed to be a very close connection between mass and energy, and this loss in mass in the synthesis of the helium nucleus from hydrogen nuclei indicates that a large amount of energy in the form of radiation has been released in the building of the helium nucleus from its components. It is easy to calculate from this loss of mass that the energy set free in forming one gramme of helium is large even compared with that liberated in the total disintegration of one gramme of radium. For example, calculation shows that the energy released in the formation of one pound of helium gas is equivalent to the energy emitted in the complete combustion of about eight thousand tons of pure carbon. It has been suggested by Eddington and Perrin that it is mainly to this source of energy that we must look to maintain the heat emission of the sun and hot stars over long periods of time. Calculations of the loss of heat from the sun show that this synthesis of helium need only take place slowly in order to maintain the present rate of radiation for periods of the order of one thousand million years. It must be acknowledged that these arguments are somewhat speculative in character, for no certain experimental evidence has yet been obtained that helium can be formed from hydrogen.

The evidence of the slow rate of stellar evolution, however, certainly indicates that the synthesis of helium, and perhaps other elements of higher atomic weight, may take place slowly in the interior of hot stars. While in the electric discharge through hydrogen at low pressure we can easily reproduce the conditions of the interior of the hottest star as far as regards the energy of motion of the electrons and hydrogen nuclei, we cannot hope to reproduce that enormous density of radiation which must exist in the interior of a giant star. For this and other

reasons it may be very difficult, or even impossible, to produce helium from hydrogen under laboratory conditions.

If this view of the great heat emission in the formation of helium be correct, it is clear that the helium nucleus is the most stable of all nuclei, for an amount of energy corresponding to three or four α particles would be required to disrupt it into its components. In addition, since the mass of the proton in nuclei is nearly 1.000 instead of its mass 1.0072 in the free state, it follows that much more energy must be put into the atom than will be liberated by its disintegration into its ultimate units. At the same time, if we consider an atom of oxygen, which may be supposed to be built up of four helium nuclei as secondary units, the change of mass, if any, in its synthesis from already formed helium nuclei is so small that we cannot yet be certain whether there will be a gain or loss of energy by its disintegration into helium nuclei, but in any case we are certain that the magnitude of the energy will be much less than for the synthesis of helium from hydrogen. Our information on this subject of energy changes in the formation or disintegration of atoms in general is as yet too uncertain and speculative to give any decided opinion on future possibilities in this direction, but I have endeavoured to outline some of the main arguments which should be taken into account.

I must now bring to an end my survey, I am afraid all too brief and inadequate, of this great period of advance in physical science. In the short time at my disposal it has been impossible for me, even if I had the knowledge, to refer to the great advances made during the period under consideration in all branches of pure and applied science. I am well aware that in some departments the progress made may justly compare with that of my own subject. In these great additions to our knowledge of the structure of matter every civilised nation has taken an active part, but we may be justly proud that this country has made many fundamental contributions. With this country I must properly include our Dominions overseas, for they have not been behindhand in their contributions to this new knowledge. It is, I am sure, a matter of pride to this country that the scientific men of our Dominions have been responsible for some of the most fundamental discoveries of this epoch, particularly in radioactivity.

This tide of advance was continuous from 1896, but there was an inevitable slackening during the War. It is a matter of good omen that, in the last few years, the old rate of progress has not only been maintained but even intensified, and there appears to be no obvious sign that this period of great advances has come to an end. There has never been a time when the enthusiasm of the scientific workers was greater, or when there was a more hopeful feeling that great advances were imminent. This feeling is no doubt in part due to the great improve-

ment during this epoch of the technical methods of attack, for problems that at one time seemed unattackable are now seen to be likely to fall before the new methods. In the main, the epoch under consideration has been an age of experiment, where the experimenter has been the pioneer in the attack on new problems. At the same time, it has been also an age of bold ideas in theory, as the Quantum Theory and the Theory of Relativity so well illustrate.

I feel it is a great privilege to have witnessed this period, which may almost be termed the Renaissance of Physics. It has been of extraordinary intellectual interest to watch the gradual unfolding of new ideas and the ever-changing methods of attack on difficult problems. It has been of great interest, too, to note the comparative simplicity of the ideas that have ultimately emerged. For example, no one could have anticipated that the general relation between the elements would prove to be of so simple a character as we now believe it to be. It is an illustration of the fact that Nature appears to work in a simple way, and that the more fundamental the problem often simpler are the conceptions needed for its explanation. The rapidity and certitude of the advance in this epoch have largely depended on the fact that it has been possible to devise experiments so that few variables were involved. For example, the study of the structure of the atom has been much facilitated by the possibility of examining the effects due to a single atom of matter, or, as in radioactivity or X-rays, of studying processes going on in the individual atom which were quite uninfluenced by external conditions.

In watching the rapidity of this tide of advance in physics I have become more and more impressed by the power of the scientific method of extending our knowledge of Nature. Experiment, directed by the disciplined imagination either of an individual, or still better, of a group of individuals of varied mental outlook, is able to achieve results which far transcend the imagination alone of the greatest natural philosopher. Experiment without imagination, or imagination without recourse to experiment, can accomplish little, but, for effective progress, a happy blend of these two powers is necessary. The unknown appears as a dense mist before the eyes of men. In penetrating this obscurity we cannot invoke the aid of supermen, but must depend on the combined efforts of a number of adequately trained ordinary men of scientific imagination. Each in his own special field of inquiry is enabled by the scientific method to penetrate a short distance, and his work reacts upon and influences the whole body of other workers. From time to time there arises an illuminating conception, based on accumulated knowledge, which lights up a large region and shows the connection between these individual efforts, so that a general advance follows. The attack begins anew on a wider front, and often with improved technical

weapons. The conception which led to this advance often appears simple and obvious when once it has been put forward. This is a common experience, and the scientific man often feels a sense of disappointment that he himself had not foreseen a development which ultimately seems so clear and inevitable.

The intellectual interest due to the rapid growth of science to-day cannot fail to act as a stimulus to young men to join in scientific investigation. In every branch of science there are numerous problems of fundamental interest and importance which await solution. We may confidently predict an accelerated rate of progress of scientific discovery, beneficial to mankind certainly in a material but possibly even more so in an intellectual sense. In order to obtain the best results certain conditions must, however, be fulfilled. It is necessary that our universities and other specific institutions should be liberally supported, so as not only to be in a position to train adequately young investigators of promise, but also to serve themselves as active centres of research. At the same time there must be a reasonable competence for those who have shown a capacity for original investigation. Not least, peace throughout the civilised world is as important for rapid scientific development as for general commercial prosperity. Indeed, science is truly international, and for progress in many directions the co-operation of nations is as essential as the co-operation of individuals. Science, no less than industry, desires a stability not yet achieved in world conditions.

There is an error far too prevalent to-day that science progresses by the demolition of former well-established theories. Such is very rarely the case. For example, it is often stated that Einstein's general theory of relativity has overthrown the work of Newton on gravitation. No statement could be farther from the truth. Their works, in fact, are hardly comparable, for they deal with different fields of thought. So far as the work of Einstein is relevant to that of Newton, it is simply a generalisation and broadening of its basis; in fact, a typical case of mathematical and physical development. In general, a great principle is not discarded but so modified that it rests on a broader and more stable basis.

It is clear that the splendid period of scientific activity which we have reviewed to-night owes much of its success and intellectual appeal to the labours of those great men in the past, who wisely laid the sure foundations on which the scientific worker builds to-day, or to quote from the words inscribed in the dome of the National Gallery, 'The works of those who have stood the test of ages have a claim to that respect and veneration to which no modern can pretend.'

ON THE ORIGIN OF SPECTRA
(RECENT PROGRESS).

ADDRESS BY

PROFESSOR J. C. McLENNAN, F.R.S.,

PRESIDENT OF THE SECTION.

I. Introduction.

THE problem of the origin of spectra is intimately bound up with that of the constitution and structure of atoms. Models of atoms of different types have been proposed from time to time, and these all have served, in a measure, to explain some at least of the chemical, optical, and mechanical properties of matter. The conception, however, that inspires and co-ordinates the whole of modern atomic physics in so far as radiation is concerned is the remarkably simple atomic model of Rutherford and Bohr.

According to this model the neutral atom consists of a central positively charged nucleus with dimensions of the same order as those of the electron itself (10^{-13} cm.),[1] and surrounded by a system of electrons whose aggregate negative charge is equal in amount to that of the positive charge carried by the nucleus. The *atomic number*—*i.e.* the number that indicates the places occupied by the element under consideration in the Periodic Table—gives for a neutral atom the number of electrons surrounding the nucleus, and is at the same time a measure of the positive electric charge carried by the latter.

Rutherford, by his brilliant experiments on the scattering of alpha rays, has shown that the electric field due to the charge on the nucleus is central, and that it follows the inverse square law practically up to the effective boundary of the nucleus. Close to the nucleus the electric field is very intense, and therefore sufficient to produce those remarkably interesting deflections of alpha rays that are being studied so widely and so successfully at the present time by the use of C. T. R. Wilson's beautiful method of photographing cloud tracks.

As regards the problem of the origin of spectra, but little progress was made so long as one limited oneself to the use of classical mechanics. With the introduction of the theory of *quanta* into the mechanics of the atom it became possible to analyse in detail the structure of atoms and to make quantitative comparisons between the properties of matter and those deducible from the different atomic models. In the developments that have taken place in this direction Niels Bohr has been the leader; but very notable and important contributions to the theory have

[1] Neuberger, *Ann. der Phys.*, Bd. 70, Heft 2, p. 139, 1923.

been made by Wilson, Sommerfeld, Ehrenfest, Kramers, Lande, and others.

Bohr in his theory supposes that each electron in an atom describes a central or quasi-central orbit under the attraction of the nucleus in combination with the fields of the other extra-nuclear electrons present in the atom. He imposes, moreover, upon these motions of the electrons in atoms something in the nature of a quantum censorship.

As a generalised postulate it is laid down that from the continuous manifold of all conceivable states of motion that may be ascribed to an atomic system there exists a definable number of stationary states that possess a peculiar stability, and that are of such a kind that every permanent change of motion within the system must involve a complete transition from one stationary state to another.

It is postulated further that while no radiation is emitted by the atomic system when it is in one of its stationary states, the process of transition from one stationary state to another is accompanied by the emission of monochromatic radiation with a frequency given by the relation

$$\nu h = E_1 - E_2,$$

where h is Planck's constant and E_1 and E_2 are the values of the energy of the atom in the initial and final stationary states between which the transition takes place. Conversely, it is to be understood that the absorption by the atomic system of radiation with the frequency given above results in a transition back from the final stationary state to the initial one. These postulates, it will be seen, form the basis of an interpretation of the laws of series spectra, for the most general of these—the combination principle of Ritz—asserts that the frequency ν of each of the lines in the spectrum of a selected element can be represented by the formula

$$\nu = T_1 - T_2,$$

where T_1 and T_2 are two spectral terms taken from a number that are characteristic of the element in question.

On Bohr's theory[2] the interpretation of the law of Ritz would be that the spectrum of the element referred to must originate in transitions between stationary states for which the atomic energy values are obtained simply by multiplying by Planck's constant the values of those spectral terms of which T_1 and T_2 are types.

This, it is evident, indicates the feasibility of establishing a connection between the series spectrum of an element and the constitution and structure of its atoms. From the spectrum of the element the series spectral terms can be selected and evaluated, and these values when multiplied by Planck's constant will give the various energy levels within and associated with the atom of the element. As the number of electrons within the said atom is given by the atomic number of the element, the problem becomes one of assigning to these constituent electrons orbits of a size and form that will provide the values of the energy levels determined by the spectral series terms.

[2] Bohr, *Nature* Supplement, July 7, 1923.

The reciprocal nature of this relationship between the series spectrum of an element and its atomic structure will be evident. In a case where the series spectrum of an element is not known a knowledge of it may be obtained by determining the energy levels in the atoms of this element independently. This can be done after the manner of Moseley and Franck and Hertz by causing atoms to emit limited portions of its spectrum under bombardment by electrons of selected speeds.

In illustration of the foregoing it may be pointed out that empirically determined spectral relationships obtained in a study of the radiation emitted by such elements as hydrogen and helium have enabled us to determine with some precision the constitution, structure, and stationary states of the atoms of these comparatively simple elements. Moreover, explicit and definite knowledge of the temporary modifications that can be impressed upon the structure of the normal atoms of these elements has been acquired through spectral relationships established by observations on the fine structure of these spectral lines, and by a study of the resolutions of these lines obtainable through the application of external electric or magnetic fields.

Stationary States—Quantum Conditions.

To illustrate the manner in which stationary states are defined on Bohr's theory we may take the simple case of an atom of hydrogen which consists of a nucleus with charge $+e$ and an electron with charge $-e$. It is known that the frequencies of the series spectra of this element are given with great accuracy by the generalised Balmer formula

$$\nu = \mathrm{K}\left(\frac{1}{n''^2} - \frac{1}{n'^2}\right) \quad . \quad . \quad . \quad . \quad (1)$$

where n'' and n' are two integers and K is the well-known Rydberg constant. From this formula we see that all the spectral terms are of the form K/n^2, and it follows at once that the energy corresponding to the various stationary states of the atom of hydrogen must be given by $\mathrm{K}h/n^2$ with n having all possible integral values.

Now it can be shown that when an electron describes an elliptic orbit about the nucleus of a hydrogen atom the major axis of the orbit described is inversely proportional to w the work required completely to remove the electron from the field of the nucleus. The major axis is, in fact, given by $2a = \frac{e^2}{w}$ If, therefore, we take $2a = \frac{e^2 n}{\mathrm{K}h}$ we have determined for the hydrogen atom a set of clearly defined stationary states consisting of a series of elliptical orbits for which the major axis takes on discrete values proportional to the squares of the whole numbers. Transitions from one to another of such a set of stationary states will suffice on Bohr's theory to account for all the lines in the series spectrum of atomic hydrogen.

In the early development of Bohr's theory it was noted that for each value of n in the equation $2a = \frac{e^2 n^2}{\mathrm{K}h}$ it was possible to have a

number of orbits with the same major axis but with different eccentricities, while all were characterised by the same energy value. For each value of n the number of such orbits was given by the number of ways in which n could be made equal to the sum of two integers, including zero. For example, if n were equal to 1 only a single orbit could exist. If n were equal to 2, then since $2=1+0$ and $2=1+1$ we could have two orbits. If n were equal to 3, we see again, since $3=3+0$. or $3=2+1$ or $3=1+2$, that we could have three orbits, &c. For each value of n there could exist a definite number of equivalent orbits. If we put $n=n_1+n_2$ it can be readily shown that the eccentricities of these equivalent orbits are given by

$$n \quad (n_1+n_2)$$

If $2b$ be taken to represent the minor axis of the different equivalent elliptical orbits, it follows that the ratio of the semi-axis is given by

$$\frac{b}{a} \quad \frac{n_1}{n} \qquad . \quad . \quad . \quad . \quad . \qquad (3)$$

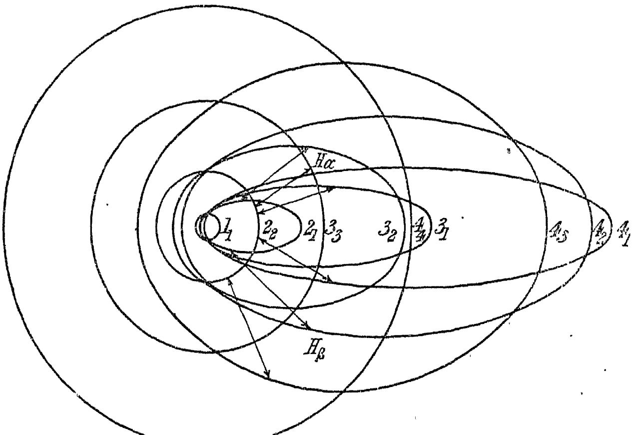


FIG. 1.—H Orbits.

Illustrations of such equivalent orbits for the hydrogen atom with differing values of n are shown in Fig 1. On this view the Lyman spectral series $\nu=K\left(1-\frac{1}{m^2}\right)$ originates in transitions to the $n=1$ orbit, the Balmer series $\nu=K\left(\frac{1}{2^2}-\frac{1}{m^2}\right)$ in transitions to either of the $n=2$ orbits, and the Paschen series $\nu=K\left(\frac{1}{3^2}-\frac{1}{m^2}\right)$ in transitions to one or other of the orbits of the $n=3$ group.

Though the single principal quantum number suffices to define the energy levels for the atom of hydrogen, the introduction of the subordinate quantum numbers n_1 and n_2 extended the basis of the theory, and, as is well known, led to developments by Sommerfeld of profound importance in dealing with the question of the fine structure of spectral lines.

Bohr's theory of the origin of spectra as it exists to-day is approached from a somewhat different angle from that given above. Through extensions initiated independently by Wilson and by Sommerfeld the quantising conditions are made to apply to momentum rather than to energy, and in dealing with the problem of the *stationary states* of a system such as that of the hydrogen atom the angular and radial momenta of the electron in its orbit are both quantised.

In more complicated systems the quantisation principle is extended to all degrees of freedom that are characteristic of the motion. The analytical conditions laid down are

$I_1 = n_1h, I_2 = n_2h \ldots\ldots I_x = n_xh$ where $n_1n_2 \ldots\ldots n_x$ are quantum integers independent of each other, and where $I_x = \int p_x d\varphi_x$ integrated over a complete cycle with reference to the generalised co-ordinates p_x and φ_x that describe the states and motions of the constituents of the system.

If we confine ourselves to the use of the two conditions $I_1 = n_1h$ and $I_2 = n_2h$, representing respectively the quantisation of the angular and radial momenta of a system consisting of a nucleus of mass M and an electron of mass m, we find that the frequencies of the radiation that can be emitted are given by

$$\nu = \frac{2\pi^2 e^4 Mm}{h^3(M+m)}\left\{\frac{1}{n''^2} - \frac{1}{n'^2}\right\} \text{ where } n'' = n_1'' + n_2'' \text{ and } n' = n'_1 + n'_2.$$

This formula possesses the advantage that it enables us to evaluate the Rydberg constant K for the spectral terms of the hydrogen spectrum, or of any system consisting of a single nucleus and one electron. It will be recalled in this connection that through the use of this formula Fowler was able to evaluate the mass of an electron from experimentally determined differences in the values of the Rydberg constant in the spectral series of hydrogen and the atom ion of helium.

Quantum Numbers and their Significance.

From the illustrations that have been given in the previous section, it will be seen that for a given atomic system the quantum numbers define the stationary states, and the energy values and moments of momentum of the system in these states. Moreover, they define the kinematical character of the electron orbits in the atomic edifice, and on account of the simple relation connecting the values of spectral terms in the series spectrum of an element with the energy of the atom of this element in its various stationary states, they define these spectral terms and enable us to calculate their values.

In the simplest possible treatment of a system such as that of the atom of hydrogen one quantum number n suffices to define the various factors just mentioned. In the theory of the fine structure of the

spectral lines of hydrogen two quantum numbers n and k were required. In the case of a series spectrum of single lines two quantum numbers n and k are requisite to define its terms and the orbits corresponding to them. For a series spectrum consisting of doublets, triplets or multiplets, three quantum numbers are required, n, k and j, to define its spectral terms and the corresponding electronic orbits. In the case of the resolution of a spectral line by the application of an external magnetic field a fourth quantum number m is necessary in order to distinguish the stationary states and to evaluate the spectral terms corresponding to the Zeeman components.

Taking the case of the stationary states associated with the outer electrons in an atom for illustration the kinematic significance of these quantum numbers is as follows: n characterises the orbit forms of these outer electrons. If $n=k$ the orbit is circular, but if $n > k$ it is elliptical, having the greater eccentricity the greater n is compared with k. The quantum number k, on the other hand, connotes kinematically a rotation of the perihelion of the elliptical orbit confined in its own plane, and on account of this turning of the perihelion the orbit takes on the form of a rosette (as shown in Fig. 5). The normal to the orbital plane about which the perihelion is progressing is called the k axis. The quantum number j indicates the total moment of momentum of the atomic state at a given instant, and the axis of this moment is called the j axis. It is in general different from the k axis, and the orbital plane performs a turning or precession about the j axis determined by the value of j the moment of momentum of the atom. If an atom endowed with the motions described above be situated in an external magnetic field, the whole system thus in motion will carry out a rotation, *i.e.* a Larmor precession about the direction of the lines of force of this magnetic field. The axis for this rotation is called the m axis, and m is a measure of the moment of momentum about it.

In spectroscopy it has become customary, in order to distinguish series of different kinds, to designate singlet systems by the use of capital letters, doublet series by Greek letters, and triplet series by small letters. Thus:

[3] P S D F = singlet systems.
π σ δ φ = doublet systems.
p s d f = triplet systems.

In the same way it has become customary to use the same letters to designate the spectral terms whose differences determine the frequencies of the lines in a series. As example we may cite 1S, 2S, &c., 1π, 2π, &c.; $1d$, $2d$, &c.; and $1f$, $2f$, &c.

Practically all efforts of spectroscopists towards arranging lines into series have had for their goal, even before the arrival of the quantum theory, in an unconscious way the establishment of the quantum numbers that define the various types of spectral terms indicated above. As a result of the progress that has been made in the last year or two, it is now generally agreed that the principal quantum number n determines the current number of the series term. For

[3] Fowler, 'Report on Series in Line Spectra.'

example, the 1S term is defined by $n=1$, the 2P term by $n=2$, the 3d term by $n=3$, and the 4F term by $n=4$, &c. The azimuthal quantum number k indicates the type to which a term belongs. For $k=1$ an s, σ or S term is signified, for $k=2$ a p, π or P term, for $k=3$ a d, δ D term, and for $k=4$ an f, φ or F term. A 3_1 term, for example, would signify a 3s, a 3 σ, or a 3S term, and a 4_2 term would be one which in spectroscopy is usually designated as a 4p, 4π or 4P term. We have then in the symbol n_k a means of defining a particular spectral term as well as a particular electronic orbit.

Principles of Selection—The Correspondence Principle.

In the early development of Bohr's theory it was found that the censorship imposed by the quantum conditions referred to above were not sufficiently drastic to account completely either for the observed complexity of the fine structure of spectral lines originating in the variation of the mass of an electron with its velocity or for the observed complexity and state of polarisation of the components of spectral lines that had their origin in the application of an external electric or magnetic field.

To make up for this deficiency arbitrary Principles of Selection, involving such factors as intensity and polarisation, were brought forward by Rubinowicz and by Sommerfeld, that found immediate and remarkable verifications in the relativity fine structure of the Balmer lines, in the Stark effect, in the Zeeman effect, and in the spectra of rotation, *i.e.* the band spectra of Deslandres.

Although these principles of selection furnished rules that have served as useful guides in unravelling the intricacies of various types of spectral resolution, it has all along been recognised by the proposers, as well as by others, that the principles as formulated rested upon a dynamical basis that was rather limited and scarcely adequate.

The whole matter, however, was given an entirely new orientation and an enhanced significance by Bohr's enunciation of the Correspondence Principle.

To elucidate this principle we may revert for a moment to the properties of the stationary orbits of the atom of hydrogen. It can be easily shown that the frequency with which the electron revolves in the nth orbit is given by

$$\omega = \frac{4\pi^2 m \mathrm{M} e^4}{(m+\mathrm{M})n^3h^3},$$

and the frequency of the light emitted when a transition occurs of the electron from the nth to n'th orbit is given by

$$\nu = \frac{2\pi^2 m\mathrm{M}e^4}{m+\mathrm{M}}\,\frac{1}{h^3}\left(\frac{1}{n'^2}-\frac{1}{n^2}\right).$$

From these two relations it follows that

$$\frac{\nu}{\omega} = \left(\frac{1}{n'^2}-\frac{1}{n^2}\right)\Big/\frac{2}{n^3}.$$

If now n and n' be taken to be large integers and not very different from each other we have

$$\nu = \Delta n \,.\, \omega \text{ numerically.}$$

As Δn must be an integer it follows that the frequencies of the light that can be emitted by the system under the conditions laid down are those of the harmonics of the frequency of the electron's orbital motion.

The explicit hypothesis made by Bohr in his Correspondence Principle is that what has been shown above to be true necessarily for very great orbital periods is also sensibly true for finite ones as well. To put the matter in another way—if the orbit described by an electron were carried out under a law of action proportional to the distance, the development of the law of motion in a Fourier series would permit the use of a fundamental term only. The Correspondence Principle would under these conditions demand that the electron could pass spontaneously only from the nth quantum orbit to the $n-1$ quantum orbit immediately below it. If these conditions were to apply in the case of the hydrogen atom, for example, it would limit each series to a single wave-length, and the Balmer series would be reduced to its first component.

The existence of series made up of numerous terms shows that the electronic orbits of an atom cannot be described under a central force varying as the direct distance, but points rather in the direction of the orbits being ellipses following approximately the Keplerian law.

In general, if the electronic motion within an atom is periodic and not simply of a pure sinusoidal character, Fourier's theory shows that the vibration of the electron is represented by a superposition of pure periodic motions that are harmonics of a fundamental one. To this classical notion there corresponds in the theory of quanta the notion of transitions from one stationary state to another with variations in the quantum number no longer equal to one only. If the Fourier series representing the motion contains effectively an harmonic of rank, 1, 2, 3 . . . or m, for example, the Correspondence Principle postulates that the atom can be the seat of transitions corresponding to differences in the characterising quantum number of 1, 2, 3 . . . or m. If on the contrary, the coefficient of a term in the Fourier series under consideration is small or equal to zero, this signifies that the probability of corresponding transitions in the atom becomes small or vanishes.

The Correspondence Principle co-ordinates every transition process between two stationary states with a corresponding harmonic vibration component in such a way that the probability of the occurrence of the transition is dependent on the amplitude of this particular vibration. On the classical theory the intensity and state of polarisation in the wave system emitted by an atom as a consequence of the existence of some vibration component are determined respectively by the amplitude and certain other characteristics of this vibration. On the quantum theory the Correspondence Principle asserts that these other special characteristics of the vibration referred to determine in an analogous manner the state of polarisation of the radiation emitted during a transi-

tion for whose occurrence the amplitude of the vibration measures the probability.

With the aid of the Correspondence Principle it has been possible to develop a complete quantum theory of the normal Zeeman effect for the hydrogen lines, and in the case of the Stark effect for these lines, where the classical theory failed to provide an explanation, the quantum theory has been so developed that it is now possible, as Kramers has shown, to account with the aid of the Correspondence Principle for the polarisation of the different components into which the lines are split, and for the characteristic intensity distribution exhibited by these components.

These and other equally interesting examples leave no doubt of the fecundity of the Correspondence Principle and of its far-reaching compass and applicability. It has endowed with precision the application of the principles of selection of Rubinowicz and Sommerfeld, and has eliminated the somewhat arbitrary formalism that has hitherto characterised them. Through its use Bohr has been able to show that the Quantum Theory can no longer be looked upon as displacing the Classical Theory, but must be considered to be a fruitful means of systematically amplifying and extending it.

The Genesis of Atoms.

One of the more interesting of the recent developments of Bohr's theory is that which concerns the genesis of atoms of different types. Bohr has put forward the view that the fundamental process that must apply consists in the successive binding of electrons one after another by a nucleus originally naked.

On this view the electrons as they are successively bound to the nucleus take up certain final and definite orbits that are characteristic of the particular atom selected in its *normal* state, and that can to a first approximation be specified by two quantum numbers—namely, the principal and subordinate quantum numbers n and k. This means that the motion of each single electron of the atomic system can be approximately described as a plane periodic motion on which is superimposed a uniform rotation in the plane of the orbit.

It is assumed as a general postulate that during the binding of an electron by a nucleus the values of the quantum numbers n and k that characterise the orbits of the earlier bound electrons remain unchanged, and that at mor . apart from a few exceptional cases, the addition of the later bound electrons merely results in slight alterations in the orientations in space of the orbits of the electrons already bound.

In arriving at his conclusions regarding the characteristics of the orbits of the bound electrons Bohr has, of course, been guided in large measure by considerations derived from a study of the arc spectra of the different elements, a type of spectrum that it is now generally agreed is emitted during the process of binding the last electron in the formation of a neutral atom. Data derivable from the characteristics of the X-ray spectra of the elements have also been utilised by Bohr to check the validity of his conclusions regarding the characteristics of the orbits of the electrons bound in neutral atoms. As X-ray lines may be considered to give evidence of stages in a process by which an

TABLE I.

ELECTRONIC ORBITS IN ATOMS OF THE ELEMENTS.

N \ n_k	1_1	2_1	2_2	3_1	3_2	3_3	4_1	4_2	4_3	4_4	5_1	5_2	5_3	5_4	5_5	6_1	6_2	6_3	6_4	6_5	6_6	7_1	7_2
1 H	1																						
2 He	2																						
3 Li	2	1																					
4 Be	2	2																					
5 B	2	2	(1)																				
— —	—	—	—																				
10 Ne	2	4	4																				
11 Na	2	4	4	1																			
12 Mg	2	4	4	2																			
13 Al	2	4	4	2	1																		
— —	—	—	—	—	—																		
18 A	2	4	4	4	4																		
19 K	2	4	4	4	4		1																
20 Ca	2	4	4	4	4		2																
21 Sc	2	4	4	4	4	1	(2)																
22 Ti	2	4	4	4	4	2	(2)																
— —	—	—	—	—	—	—	—																
29 Cu	2	4	4	6	6	6	1																
30 Zn	2	4	4	6	6	6	2																
31 Ga	2	4	4	6	6	6	2	1															
— —	—	—	—	—	—	—	—	—															
36 Kr	2	4	4	6	6	6	4	4															
37 Rb	2	4	4	6	6	6	4	4			1												
38 Sr	2	4	4	6	6	6	4	4			2												
39 Y	2	4	4	6	6	6	4	4	1		(2)												
40 Zr	2	4	4	6	6	6	4	4	2		(2)												
— —	—	—	—	—	—	—	—	—	—		—												
47 Ag	2	4	4	6	6	6	6	6	6		1												
48 Cd	2	4	4	6	6	6	6	6	6		2												
49 In	2	4	4	6	6	6	6	6	6		2	1											
— —	—	—	—	—	—	—	—	—	—		—	—											
54 X	2	4	4	6	6	6	6	6	6		4	4											
55 Cs	2	4	4	6	6	6	6	6	6		4	4				1							
56 Ba	2	4	4	6	6	6	6	6	6		4	4				2							
57 La	2	4	4	6	6	6	6	6	6		4	4	1			(2)							
58 Ce	2	4	4	6	6	6	6	6	6	1	4	4	1			(2)							
59 Pr	2	4	4	6	6	6	6	6	6	2	4	4	1			(2)							
— —	—	—	—	—	—	—	—	—	—	—	—	—	—			—							
71 Lu	2	4	4	6	6	6	8	8	8	8	4	4	1			(2)							
72 Hf	2	4	4	6	6	6	8	8	8	8	4	4	2			(2)							
— —	—	—	—	—	—	—	—	—	—	—	—	—	—			—							
79 Au	2	4	4	6	6	6	8	8	8	8	6	6	6			1							
80 Hg	2	4	4	6	6	6	8	8	8	8	6	6	6			2							
81 Tl	2	4	4	6	6	6	8	8	8	8	6	6	6			2	1						
— —	—	—	—	—	—	—	—	—	—	—	—	—	—	—		—	—						
86 Nt	2	4	4	6	6	6	8	8	8	8	6	6	6			4	4						
87 —	2	4	4	6	6	6	8	8	8	8	6	6	6			4	4					1	
88 Ra	2	4	4	6	6	6	8	8	8	8	6	6	6			4	4					2	
89 Ac	2	4	4	6	6	6	8	8	8	8	6	6	6			4	4	1				(2)	
90 Th	2	4	4	6	6	6	8	8	8	8	6	6	6			4	4	2				(2)	
— —	—	—	—	—	—	—	—	—	—	—	—	—	—			—	—	—				—	
— —	—	—	—	—	—	—	—	—	—	—	—	—	—	—		—	—	—				—	—
118 ?	2	4	4	6	6	6	8	8	8	8	8	8	8	8		6	6	6				4	4

atom undergoes *reorganisation* after a disturbance in its interior, the energy levels obtainable for a neutral atom from the values of the frequencies of its X-radiation must agree with those representing the final orbits provided for this atom by the characteristics of its own arc spectrum as well as by those of the arc spectra of its ions or of the elements of lower atomic number.

As stated above, the basis of Bohr's classification of the orbits of atoms in their normal state is largely of an experimental character. It is not altogether so, however, for he has been able in the case of a number of the simpler atoms to work out the relative stabilities of orbits that are conceivable ones for these atoms, and by the use of the quantum theory, supported by the Correspondence Principle, has obtained a theoretical justification for the classification that he has adopted.

The results of Bohr's work in this direction are given in Table I., where N denotes the atomic number and n and k give the values of the principal and subordinate quantum numbers respectively of the orbits indicated. According to the scheme, it will be seen, the orbits are divided into groups corresponding to the various values of the principal quantum number n, and into sub-groups designated by different values of the subscript quantum number k. While orbits for which n has the value 1 are all of one type those for which n has the value 2 are of two types, those for which n has the value 3 are of three types, and so on.

Illustrations of the structure of a number of neutral atoms are given by the diagrams on Plates I. and II. These have been copied from a paper by Kramers[4] that has recently appeared, and are stated to be similar to those prepared by Bohr for use in his own lectures. The electron orbits in the neutral atoms selected are arranged in groups from the centre of the atom outwards according to increasing values of the principal quantum number. The diagrams do not take account of the rotation of the orbits in their own plane, nor in the case of the heavier atoms is there any attempt to indicate the characteristics of the orbits close to the nucleus. They merely serve to illustrate in a general way Bohr's ideas regarding the genesis of atoms. A characteristic feature of the scheme is brought out by the illustrations of the orbits of the atoms of the rare gases. These, it will be seen, provide for the recurrence of the structure of a lighter atom as a constituent part of the structure of each of the heavier ones.

An illustration given by Bohr of the process of binding an electron to a nucleus is shown in Fig. 2. In this diagram the representation is that of the stationary states corresponding to the emission of the arc spectrum of potassium. No attempt is made to depict the duplex character of each of the stationary states. The curves show the form of the orbits described in the stationary states by the last electron captured in the potassium atom. They can be considered to represent stages in the process whereby the 19th electron is bound after eighteen previous electrons have already been bound in their normal orbits. The orbits are marked with the symbol n_k where n and k are respectively the principal and subordinate quantum numbers.

[4] Kramers, *Die Naturwissenschaften*, Heft 27, July 6, 1923.

The states 4_1 5_1 6_1 . . . are to be considered as those which give rise to the σ terms in the series arc spectrum of potassium. The states 4_2 5_2 6_2 connote the π spectral terms, and the states 3_3, 4_3, the δ spectral terms. The state 4_4 will give rise to one of the ϕ or fundamental terms in the series spectrum.

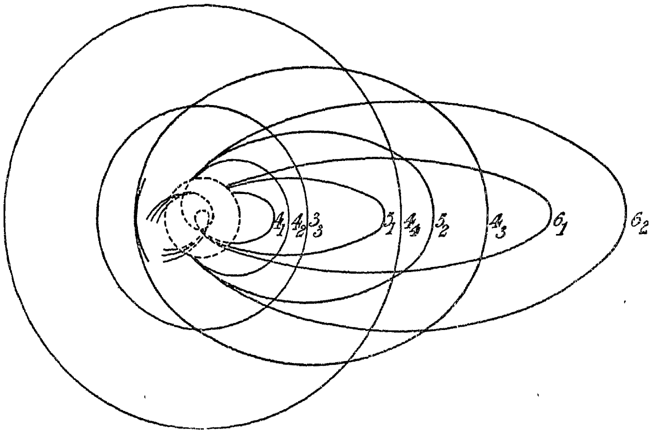


FIG. 2.—Binding Potassium Orbits.

Grotrian's method of exhibiting these relationships is instructive. Its application to the case of the stationary orbits of the potassium atom is shown in Fig. 3.

A few outstanding features of the classification give in Table I. may be referred to. In the first place, the scheme provides for periodicity in the properties of the elements. For example, in the case of

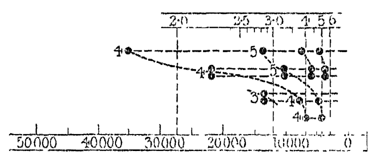
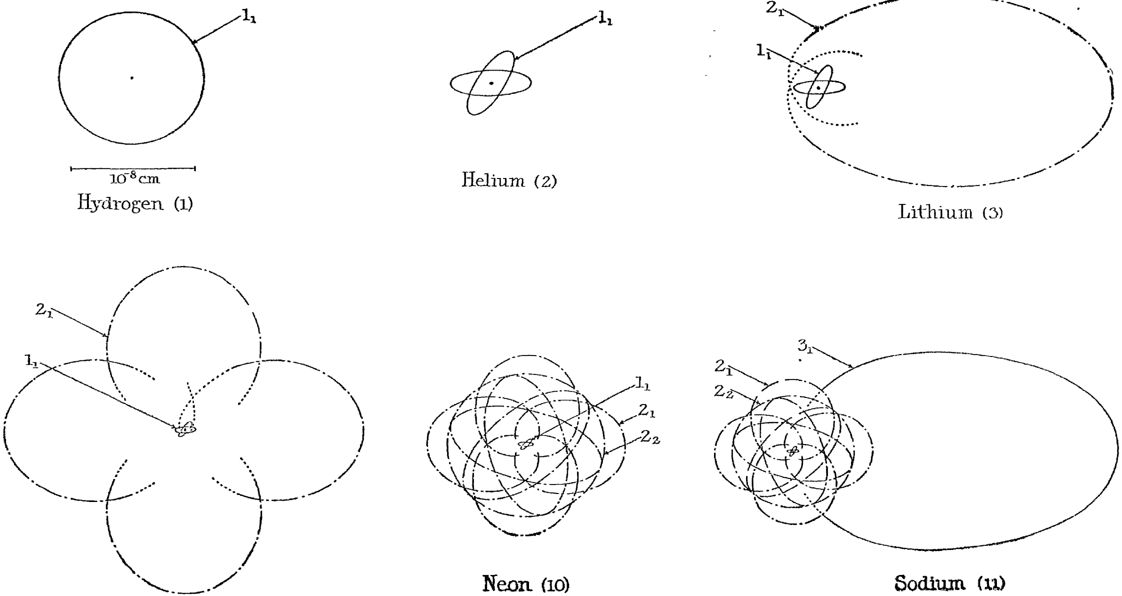


FIG. 3.—Grotrian Diagram.

the heavier inert gases the outer group of electrons is made up of two sub-groups with four electronic orbits of the same type in each. For these sub-groups the subordinate quantum number has the values 1 and 2. The principal quantum number increases by unity from element to element. Again, in the case of the alkali elements the outer group contains but one orbit. For it the subordinate quantum number k has the characteristic value 1, and the principal quantum number again



Illustrating the Address on the Origin of Spectra.

increases by unity as we pass from a lighter to a heavier element in the alkali group.

Another interesting feature of the classification is that in the genesis of the different kinds of atoms provision is made for the appearance at certain stages of sets of homologous elements such as those of the iron, palladium, platinum, and rare-earth groups. For example, the appearance of the iron group accompanies the establishment in the normal atom of an inner group of orbits of the 3_3 type beginning with the element scandium. These 3_3 orbits begin in the fourth period and differentiate it from the second and third because for the first time the charge on the nucleus is sufficiently great to make it possible for the successive atoms to differ by an extra electron in such an inner group instead of in an outer one. The appearance of the palladium group also is associated with the beginning of a development of inner orbits of the 4_3 type at a stage in the binding process when the outer orbits of the next lighter atoms consist of 5_1 quantum orbits. The appearance of the platinum and rare-earth groups of elements, too, it will be seen, is associated with the beginnings of developments of inner orbits of the 5_3 and 4_4 types respectively at stages in the binding process when the outer orbits of the neighbouring lighter elements are of the 6_1 type.

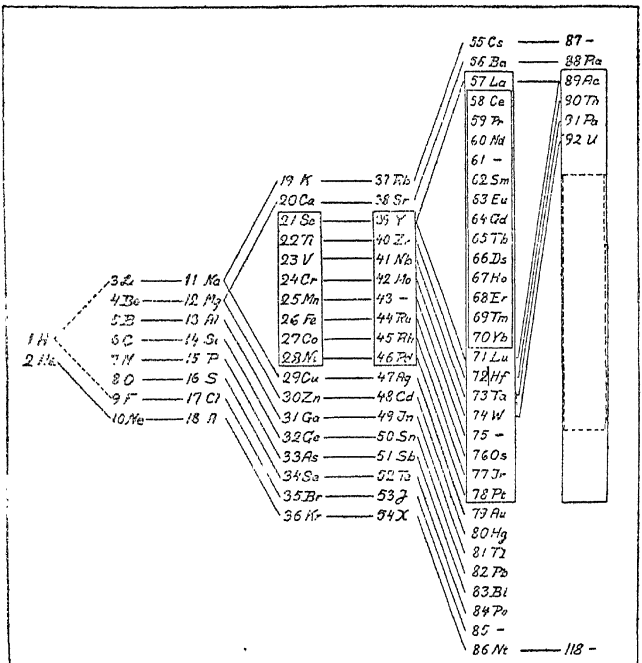
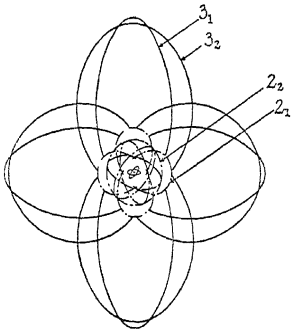
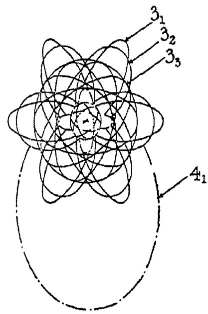


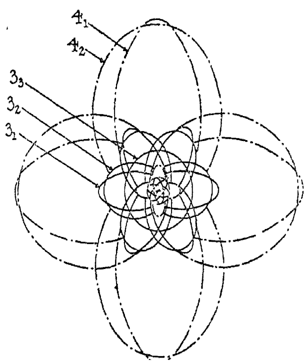
FIG. 4.—Elements,



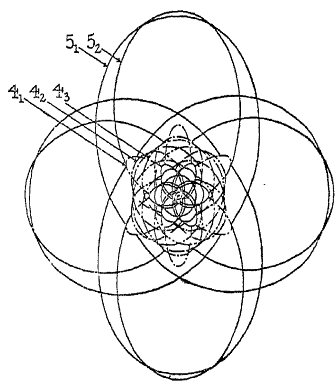
Argon (18)



Copper (29)



Krypton (36)



Xenon (54)

Illustrating the Address on the Origin of Spectra.

These and other features of the classification that might be referred to are illustrated by the arrangement of the elements shown in Fig. 4. In this representation, it will be noted, those elements that belong to the same period are given in vertical columns, and those that from their chemical and optical properties can be considered homologous are connected with one another by straight lines. Groups of elements that possess analogous physical properties, and that differ from one another by variations in the number of electrons belonging to inner groups, are enclosed, as the diagram shows, by rectangular spacings.

Peculiar interest attaches to the newly discovered element of atomic number 72, to which the name 'Hafnium '[5] has been given. Conditions imposed by the quantum theory, in Bohr's view, make it imperative to assign this element to the platinum group instead of to the rare-earth group, as Dauvillier[6] and others have suggested. Theoretically, this element would appear to be a homologue of zirconium, and it is interesting to note that Coster and Henesy, who have been chiefly concerned with its discovery, have been able to obtain from zircon-bearing minerals considerable quantities of a substance whose chemical properties are similar to those of zirconium, and whose X-ray spectrum is that of an element with atomic number 72.

In the remainder of my address I propose, with your permission, to deal with a number of matters that are closely associated with developments of the quantum theory of the origin of spectra and that appear to merit some special attention and consideration at the present time.

The Fine Structure of the Balmer Lines of Hydrogen.

In the simplest treatment by the quantum theory of the origin of the spectrum of atomic hydrogen no allowance is made for a variation in the mass of the electron with its speed. If this factor be taken into account, as it has been by Sommerfeld, it is found that the motion of the electron is reducible to a motion in an elliptic orbit upon which is imposed a slow rotation in its own plane about the nucleus as focus. The resulting orbit has the form of a rosette, and is similar to that shown in Fig. 5.

In this treatment the chief factor in determining the stationary states is the principal quantum number n, but the subordinate quantum number k is also contributory. The former practically determines the major axis and the period of the elliptical orbit, while the latter defines the parameter of the ellipse—*i.e.* the shortest chord through its focus. The subordinate quantum number k also determines the period of rotation of the elliptic orbit in its plane. The energy corresponding to each stationary state is in the main determined by the value of the quantum number n, but stationary states determined by the same value of n are characterised by energy values that vary slightly with different values of the quantum number k.

[5] Coster and Henesy, *Nature*, Jan. 20, Feb. 10, 24, and April 7, 1923.

[6] Dauvillier, *C.R.*, t. 174, p. 1347, May 1922; Urbain, *C.R.*, t. 174, p. 1349, May 1922, and t. 152, p. 141, 1911.

The diagram shown in Fig. 1 represents an instantaneous aspect of the orbits of the different stationary states, and the designations n_k give the values of the quantum numbers characterising the different orbits.

According to this treatment each of the numbers of the Balmer series

$$= K\left(\frac{1}{n^2} - \frac{1}{m^2}\right)$$

should consist of a doublet, and each of the components of these doublets should possess a fine structure. Calculations made by Sommerfeld showed that the frequency difference for these doublets should be constant over the whole of the Balmer series, and should be equal to 0.36cm.$^{-1}$.

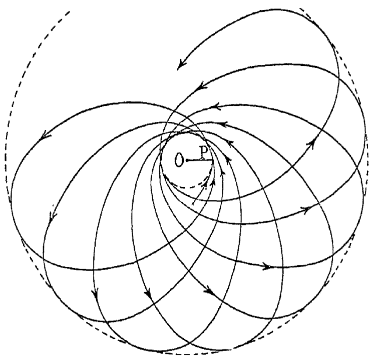


FIG. 5.—Rosette.

As the theory applies equally well to the corresponding series in the spectrum of positively charged helium, the doublets of this series were investigated by Paschen, and were found to have separations that led to a value of 0.3645 ± 0.0045 for the frequency difference of the doublets of the hydrogen Balmer series.

Since the publication of Paschen's work on the helium doublets a number of investigators[7] have attempted, from measurements on the

[7] Michelson and Morley, *Phil. Mag.*, vol. 24, p. 46, 1887.
Ebert, *Wied. Ann.*, vol. 43, p. 800, 1891.
Michelson, *Bur. Int. des Poids et Mesures*, vol. 11, p. 139, 1895.
Houston, *Phil. Mag.*, vol. 7, p. 460, 1904.
Fabry and Buisson, *C.R.*, vol. 154, p. 1501, 1912.
Paschen, *Ann. der Phys.*, vol. 50, p. 933, 1916.
Merton and Nicholson, *Roy. Soc. Proc.*, A, vol. 93, p. 28, 1917.
Merton, *Roy. Soc. Proc.*, A, vol. 87, p. 307, 1920.
Gehrcke and Lau, *Phys. Zeit.*, vol. 21, p. 634, 1920.
McLennan and Lowe, *Roy. Soc. Proc.*, A, vol. 100, p. 217, 1921.
Gehrcke and Lau, *Phys. Zeit.*, vol. 22, p. 556, 1921.
Oldenburg, *Ann. der Phys.*, vol. 67, p. 69, 1922.
Gehrcke and Lau, *Ann. der Phys.*, vol. 67, p. 388, 1922.
Oldenburg, *Ann. der Phys.*, vol. 67, p. 253, 1922.
Geddes, *Proc. Roy. Soc. Edin.*, vol. 43, p. 37, 1923.

separations of H_α and H_β, and in some cases of H_γ and H_δ, to look for evidence that would lead to a confirmation of Sommerfeld's theory. Up to the present the results obtained could not be considered as satisfactory. There was a lack of agreement in the values obtained for the separations by different investigators, and on the whole the values obtained were less than that demanded by the theory. The matter was reinvestigated recently, at my suggestion, by one of the research workers in the Physical Laboratory of the University of Toronto, Mr. G. M. Shrum, and in his experiments he succeeded in eliminating practically the whole of the secondary spectrum, and as a result was able to include in his measurements of the doublet separations that of H_ϵ as well as those of H_α, H_β, H_γ, and H_δ.

The results are the following:—

Line	Wave-length	Separation of the Components		Probable Error
		$d\lambda$	$d\nu$	
H	6562·79 A	0·143 A	0·33 cm.$^{-1}$	0·02 cm.$^{-1}$
H_β	4861·33 „	0·085 „	0·36 „	0·01 „
H_γ	4340·46 „	0·0[illegible]0 „	0·37 „	0·02 „
H_δ	4101·73 „	0·061 „	0·36 „	0·02 „
H_ϵ	3970·07 „	0·055 „	0·35 „	0·02 „

It will be seen that as far as the doublet separations are concerned they afford a striking confirmation of Sommerfeld's theory.

Model of the Atom of Helium.

Considerable interest attaches to the atom of Helium. From the chemical point of view it has been considered to be inert, and consequently not likely to enter into chemical combination. Of all atoms it is the most stable, for it has the highest ionisation potential, namely 24.5 volts. A study of the X-radiation emitted by the elements generally makes it appear that the configuration we assign to the electronic orbits in helium atoms is maintained intact throughout the whole of the remaining heavier elements. These orbits, as Table I. shows, constitute for all atoms the K X-ray group the innermost and most stable system. For these reasons it is highly desirable that a model of the atom of helium be realised possessing high stability endowed with the capacity to emit radiation exhibiting the characteristic features of the helium spectrum, and having energy values for its normal and temporary stationary states that fit in with the experimentally determined values of its ionisation, resonance, and other critical excitation potentials.

The earlier models of the atom of helium put forward failed entirely to meet these requirements. Models recently conceived by Lande[8] and by Bohr[9] are at the present time receiving considerable attention. In these the two electrons in the normal atom are taken to move in equiva-

[8] Lande, *Phys. Zeit.*, No. 20, p. 228, 1919.
[9] Bohr, *Zeit. für Phys.*, No. 2, p. 464, 1920.

lent 1_1 orbits. As a first approximation these may be described as circular orbits with planes inclined at an angle to each other. Bohr assumes this angle to be 120°, and on account of the interaction between the two electrons the two orbits are supposed to be slowly turning about a fixed momentum axis in the atom. A diagrammatic representation of this model is shown in Fig. 6.

Such a model, however, will not account for the whole of the spectrum of helium, which is known to consist of two complete but separate sets of series, the one being made up of single lines and the other of doublets. An important feature of the spectrum of helium, too, is that it contains no lines that are the result of combinations between spectral terms belonging to one of the sets of series and those belonging to the other. The explanation put forward is that while helium in its normal state exists in the form of atoms with crossed orbits, designated by the name parhelium, it can also exist in a metastable form, known as orthohelium, as well. In the latter state the electronic orbits are supposed to be in the same plane with the electrons revolving in the same direction. In the most stable form of ortho-

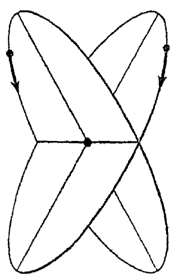


FIG. 6.—Helium Model.

helium one of the electrons is supposed to move in a 1_1 orbit and the second in a 2_1 orbit. The singlet series in the spectrum of helium are assigned to parhelium and the doublet series to orthohelium.

If parhelium be bombarded by electrons it appears to be possible to transform its atoms into the metastable form, but once the atoms are in the latter state it does not seem to be possible for them to revert directly to the normal form by means of a simple transition accompanied by the emission of radiation. They can only do so by a process analogous to a chemical reaction involving interaction with atoms of other elements.

The fact that helium, under certain conditions, can be made to emit a band spectrum in addition to its line spectrum connotes the possibility of helium existing in the molecular form. Since helium in the form of orthohelium has its outer electron in a 2_1 orbit, the atoms of orthohelium in so far as chemical combination is concerned occupy a position analogous to atoms of lithium, which also possess a 2_1 orbit in their normal state. As this feature enables lithium to react

vigorously with other atoms, one would expect orthohelium also to be capable of entering into chemical combinations. From this it would appear that molecular helium originates in atoms that have undergone a transition into the metastable state. As to the atoms of parhelium, there appears to be no warrant of this or any other character for supposing that they can participate in any kind of chemical union.

The view just presented has gained strong support from Frank and Knipping's experiments on the excitation potentials of helium atoms by electronic bombardment, and by Lyman's recent work on the extreme ultra-violet spectrum of helium, in which it has been shown that radiation of the wave-lengths 600.5 A, 584.4 A, 537.1 A, 522.3 A, and 515.7 A are absorbed by helium in its normal state. The scheme[10] set forth in Fig. 7 and the data collated in Table II. are self-explanatory, and show how on the view just put forward the radiation whose wave-lengths were measured by Lyman can originate, and how the excitation potentials observed by Frank and Knipping can be realised.

According to this scheme electrons with a speed corresponding to a potential of 19.75 volts will be able to transform parhelium into orthohelium, and those with speeds corresponding to 20.55 volts and 21.2 volts will be able to lift the electrons from 1, S orbits to 2, S and 2, P orbits respectively. Under bombardment by electrons with speeds the equivalent of 24.5 volts the helium atoms will be ionised. The scheme shown in Fig. 7 also indicates how the series spectrum of orthohelium originates.

The considerations set forth above would seem to clear up some of the difficulties that have hitherto been encountered in realising a satisfactory model of the helium atom, and in reaching an explanation of the origin of the radiation that atoms of helium can emit. The complete solution of the problem, however, has received a set-back from the results of an investigation recently carried out by Kramers,[11] for according to his calculations the ionisation potential of the crossed orbit model comes out 3.8 volts less than the experimentally determined value. His calculations also show that in a mechanical sense the crossed orbit model cannot be considered to be a stable one. Although real progress has been made, it cannot be said that finality has been reached in the determination of the form of a completely satisfactory model of the atom of so simple an element as helium.

A somewhat novel aspect of the problem has recently been emphasised by Silberstein.[12] He assumes the crossed orbit model of the helium model to be capable of taking up a number of stationary states with the planes of the orbits at a series of angles other than 120°.On this basis he has been able, by taking for granted the dynamical legitimacy of the crossed orbit system, to calculate values for the ionisation and other excitation potentials that are in remarkably good agreement with the experimental values found by Frank and Knipping, Horton, and others.

[10] Grotrian, *Die Naturwissenschaften*, Heft 17, p. 321, 1923.
[11] H. A. Kramers, *Zeit. für Phys.*, vol. 13, p. 339, 1923.
[12] Silberstein, *Nature*, Ap. 28, p. 567, 1923, and July 14, p. 53, 1923.

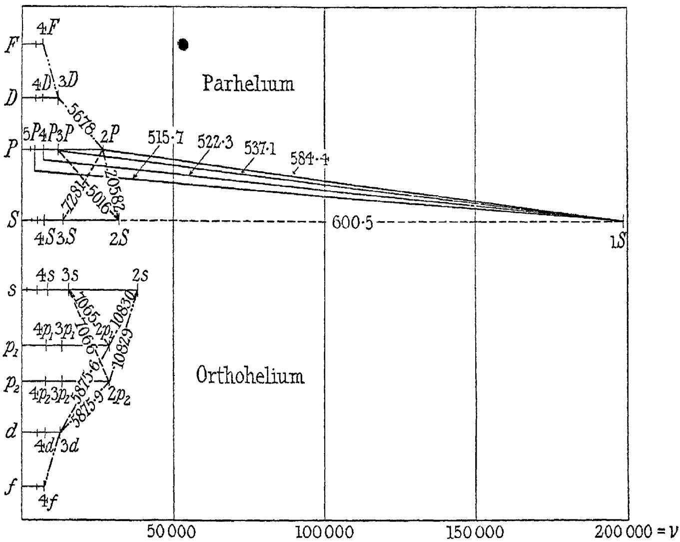


FIG. 7.—Scheme of He Lines.

TABLE II.

Lines Observed by Lyman	Series Representation	Calculated Excitation Potentials	F. & K.'s Measured Excitation Potentials	Remarks
—	1, S—2, *s*	19·77	19·75	Transition voltage connoting change from parhelium to orthohelium.
600·5 A	2, S—1, S	20·55	20·55	A weak radiation and one not provided for by the principle of selection.
584·4 „	2, P—1, S	21·12	21·2	First member of absorption series of parhelium.
537·1 „	3, P—1, S	22·97	22·9	Second member of absorption series of parhelium.
522·3 „	4, P—1, S	23·62	—	Third member, &c.
515·7 „	5, P—1, S	23·92	—	Fourth member, &c.
502·0 „ (calculated)	— —1, S	24·5	24·6	Series limit and ionisation potential.

Resonance and Ionisation Potentials.

The results of investigations on the absorption spectra of zinc, cadmium, and mercury, and on the resonance and ionisation potentials of these elements, have shown that for this group of elements the ionisation potentials are given by $V = h\nu/e$, where ν is the frequency denoted by (n, S), namely, that of the last member of the series $\nu = (n, S) - (m, P)$.[13] It is also known that their resonance potentials are given by the same relation with ν having the value $(n,S) - (2,p_2)$ the frequency of the first member of a combination series. In the case of the alkaline earths similar relations obtain. With the alkali elements the frequencies that determine the resonance and ionisation potentials are given by $\nu = (n, \sigma) - (n, \pi)$[14] and $\nu = (n, \sigma)$ respectively. It is, therefore, clear from the characteristics of the spectral terms involved that, while the electron concerned in phenomena associated with resonance and ionisation potentials must be the one that is most easily displaced in or removed from the atom, this electron must be bound in atoms of the elements mentioned—when these are in their normal state—in orbits of the n_1 type, *i.e.* in orbits for which the subordinate quantum number has the value 1. Now, a reference to Table I. will show that this characteristic is exactly the one possessed by the electron that is last bound in the atoms of the elements cited.

It follows, then, that if we know the type of orbit occupied by the last bound electron in the normal atom of any element we can at once deduce the type of the series whose first and last members will enable us to calculate the resonance and ionisation potentials of the element. Moreover, the wave-lengths of such a series will be the ones that will be selectively absorbed by the vapour of the element, provided its temperature is sufficiently low to ensure that the atoms constituting the vapour are in their normal state.

Previous to the publication by Bohr of the scheme in Table I. it had been thought that for all elements the resonance and ionisation potentials should be obtainable from spectral frequencies of the $(n, \sigma) - (m, \pi)$ or $(n, S) - (m, P)$ type. Numerous attempts were made by investigators of the absorption spectra of such elements as thallium, lead, tin, &c., to group the wave-lengths of the radiation absorbed into a principal series that would enable one to calculate the critical potentials for these elements. These efforts, however, ended in failure, for though wave-lengths were found that were selectively absorbed by the vapours of the elements referred to, and though it was found possible to fit these partially at least into series, it was clear that the series obtained did not satisfy the conditions demanded by series of the principal type.

By the publication of Bohr's scheme of atomic orbits, however, it became evident that since in the case of the aluminium group of elements, for example, the electron last acquired in making the atoms neutral is bound in an orbit of the n_2 type, the first member of the

[13] According to Bohr's scheme n has the value 4 for Zn, 5 for Cd, and 6 for Hg, while m has the value 2.

[14] In this formula n has the value 2 for Li, 3 for Na, 4 for K, 5 for Rb, and 6 for Cs.

spectral series that would enable us to calculate the resonance and ionisation potentials for this group of elements must be of the type $\nu=(n, \pi) - (m, x)$ and not of the type $\nu=(n, \sigma) - (m, \pi)$. Moreover, this makes it clear that the series of wave-lengths that should be selectively absorbed by vapours in the normal state of the elements of the aluminium group would be the first and second subordinate ones represented by $\nu=(n, \pi)-(m, \sigma)$ and $\nu=(n, \pi)-(m, \delta)$.

Recent experiments by Carroll[15] and by Grotrian,[16] as well as earlier ones by Wood and Guthrie[17] and by the writer,[18] show that the wave-lengths most readily absorbed by non-luminous thallium vapour all belong to the sharp or diffuse subordinate series in the spectrum of this element. With indium vapour Grotrian has obtained similar results. With aluminium, as with thallium, the wave-lengths absorbed by the comparatively cool vapour that surrounds an electric arc in the metal belong to the sharp and diffuse subordinate series.

From these results it is clear that the evidence furnished by spectral data amply confirms the view put forward by Bohr that in the case of the heavier elements of the aluminium group at least, the electron last acquired by the neutral atom of the respective elements is bound in an orbit of the n_2 type. In the case of the light element boron, the series data available are so meagre that it is not possible as yet to affirm that the same law applies.

From the known values of the frequency $\nu=(n, \pi_2)$ in the spectra of the elements aluminium, gallium, indium, and thallium, it follows that the resonance potentials for these elements are respectively 3.12 v., 3.08 v., 3.00 v., and 3.26 v., and that the ionisation potentials are respectively 5.94 v., 5.96 v., 5.75 v., and 6.07 v.

For thallium, the only element of this group as yet investigated by the method of electronic impact, Foote and Mohler found the resonance and ionisation potentials to have the respective values 1.07 v. and 7.3 v. The agreement, it will be seen, is not very close. It should be noted, however, that Foote and Mohler, in giving their results, indicate that they should be considered to be approximate only.

Electronic Orbits of the Atoms of the Lead-Tin Group.

It will be seen that the scheme of orbits given in Table I. makes no provision for the elements of the Lead-Tin group. The reason for this is that hitherto but little spectroscopic data have been available for these elements. Besides, the development of the quantum theory does not appear to have been sufficiently advanced to include the atoms of these elements within the scope of its application. Progress with these elements is, however, now possible owing to the fact that Thorsen[19] has been able to organise a part of the spectrum of lead into a triplet set of first and second subordinate series. These series

15 Carroll, *Proc. Roy. Soc.*, Series A, vol. 103, p. 334, May 1923.

16 Grotrian, *Zeit. für Phys.*, Bd. 12, p. 218, 1923.

17 Wood and Guthrie, *Ast. Phys. Jl.*, vol. 29, p. 211, 1909.

18 McLennan, Young and Ireton, *Proc. Roy. Soc. of Canada*, Section III., p. 7, 1919.

19 Thorsen, *Die Naturwissenschaften*, Heft 5, Feb. 2, 1923. Recent experiments by Thorsen lead to the value 9.18 v. for the ionisation potential of gold.

have frequencies given by $\nu=(n, p_x)-(m, s)$[20] and $\nu=(n, p_x)-(m, d)$ where x has the values 1, 2, 3. In all about 54 wave-lengths have been allocated into places in these series. In this connection it is of importance to note that Thorsen does not seem to have been able to assign any of the wave-lengths in the lead spectrum to a related principal series.

Following up this work, Grotrian[21] has recently pointed out that of the wave-lengths known to be selectively absorbed by non-luminous lead vapour,[22] the prominent ones $\lambda=2833$ A, $\lambda=2170$ A, $\lambda=2053.8$ A, and $\lambda=3683$ A were not included in the series formulated by Thorsen. He has been able to show, further, that they can be included in a more extended scheme of first and second subordinate series that includes, in addition to those of Thorsen, two others that have for their highest frequencies $\nu=(2, p_4)=59826\text{cm}^{-1}$ and $\nu=2, p_5=51677\text{ cm}^{-1}$. According to this scheme $\lambda=2833$ A would have the frequency $\nu=(2,p_4)-(2,s)$, $\lambda=2053$ A the frequency $\nu=(2,p_4)-(3,s)$, $\lambda=2170$ A the frequency $\nu=(2,p_4)-(3, d_2)$, and $\lambda=3683$ A the frequency $(2, p_5)-(2, s.)$ These results lead at once to definite conclusions regarding the outermost orbit in the normal atoms of lead. Since all the wave-lengths absorbed are members of subordinate series, it follows that the electron last acquired by a neutral atom of lead must be bound in an orbit for which the subordinate quantum number k has the value 2. This leads to the conclusion that the scheme of orbits for lead will include two of the 6_1 type and two of the 6_2 type.

From the frequencies

$$\nu=(2, p_4)-(2, s)=35296\text{cm}^{-1} \text{ and } \nu=(2, p_4)=59826\text{cm}^{-1}$$

it follows that the resonance and ionisation potentials of lead should be respectively 4.35 v. and 7.4 v. As Foote and Mohler have found by the method of electronic impact these critical potentials to be 1.26 v. and 7.93 v., it will be seen that while the values for the resonance potentials show no agreement, there is a fair agreement in the case of the values of the ionisation potentials.

As very little is known about the series spectra of tin[23] and germanium, one cannot as yet write with precision about the outermost orbits of the normal atoms of these elements. Considerations of periodicity make it highly probable that they will be of the same type as those of lead. This would mean that tin should have its two outermost electrons bound in the normal atoms of equivalent 5_2 orbits, and the normal atoms of germanium their two outermost electrons bound in 4_2 orbits. The results obtained with the series spectra of lead will no doubt lead immediately to the organisation of the spectral lines of tin and germanium into series.

Though but little has been published about the series spectra of neutral atoms of silicon, Fowler reports that he has been able to show that the arc spectrum of this element includes a number of related

[20] According to Thorsen n has the effective value 2 in this formula.

[21] Grotrian, *Die Naturwissenschaften,* Heft 13, March 30, 1923.

[22] McLennan and Zumstein, *Proc. Roy. Soc. of Canada,* Section III., vol. xiv., p. 9, 1920.

[23] The writer has been able to show recently that the spectrum of tin includes series of the same type as those of lead.

triplets. In this regard it is analogous to the spectrum of lead as originally classified into series by Thorsen. From general considerations the existence of these triplet series would connote that there are two valency electrons in the atoms of silicon. As the outermost electron in normal atoms of aluminium is bound in a 3_2 orbit, the two outermost electrons in the normal atoms of silicon would appear to be bound in equivalent orbits of this type.

As to normal atoms of carbon, Bohr has expressed the opinion that the four last bound electrons may be expected to form an exceptionally symmetrical configuration, in which the normals to the planes of the orbits occupy positions relative to one another nearly the same as the lines from the centre to the vertices of a regular tetrahedron. Such a configuration would, it is evident, furnish a suitable foundation for explaining the structure of organic compounds. Thus, considerations of symmetry would undoubtedly lead to the view that the four outer electrons in carbon atoms were all bound in 2_1 quantum orbits symmetrically arranged in space.

This scheme of outer orbits is radically different from that ascribed above to the outer orbits of the atoms of lead, tin, germanium, and silicon, and the explanation of the difference is at yet not at all clear.

The fact that the spectrum of lead has been shown to include at least five sharp subordinate series and four diffuse subordinate series suggests in a measure a parallel to the spectrum of neon for which Paschen [24] has identified at least thirty sharp series and seventy-two diffuse series. Multiple series of this character have also been shown by Meissner [25] and by Nissen [26] to be included in the spectrum of argon. Though this parallel might be taken to indicate that the orbits of the four last bound electrons in the atoms of lead and in those of the allied elements are all of the n_1 type, it would seem that since the wave-lengths selectively absorbed by lead vapour all belong to subordinate series, we must conclude that in the case of lead at least its outermost orbits must be two in number and of the 6_2 type.

The Kossel-Sommerfeld Displacement Law.

I have stated that Bohr in arriving at his scheme of atomic orbits was guided by the view that the fundamental process to keep in mind was that when a nucleus originally naked acquired electrons sufficient in number to neutralise its charge, it did so by binding them according to a programme that was definite and fixed for each value of the nuclear charge.

If this view be accepted, it follows that if we were to detach from the neutral atom of an element its most loosely bound electron, we should expect to find that the orbits which remained were characterised by the same quantum numbers as defined them in the neutral atom. Moreover, except in certain special cases, these orbits would be identical in type with those of the neutral atoms of the next lighter element.

[24] Paschen-Götze, *Seriengesetze der Linienspektren*, p. 30.
[25] Meissner, *Ann. d. Phys.*, Bd. 51, p. 95, 1915.
[26] Nissen, *Phys. Zeit.*, vol. 21, p. 25, 1920.

The exceptional cases would include those elements whose atomic structure involved the commencement of the development of an inner system of orbits, such as those of the 3_3, 4_3, 4_4, &c., groups. Subject to these limitations, we should expect to find that if the n last-bound electrons were removed from a neutral atom of an element the orbits that remained in this atom would be identical in type with those of the neutral atoms of the nth lighter element. This would mean that the arc spectrum of the monovalent positive ion of arc element would be identical as to types of series involved with the arc spectrum of the neutral atoms of the next lighter element. There would be this difference, however, that in the series formulæ of the spectrum of the ion the Rydberg constant would be 4K, whereas in the series of the spectrum of the neutral atoms of the lighter element it would be K. Putting the matter as it is ordinarily stated, the spark spectrum of an element should be made up of series of the same type as those of the arc spectrum of the next lighter element. This is known as the Kossel-Sommerfeld Displacement Law.

Numerous illustrations of this law might be cited. For example, the series in the spectrum of the monovalent positive helium ion are of the same type as those of the spectrum of atomic hydrogen. Again, the series in the spark spectra of the alkali elements have been shown to be similar in type to those of the arc spectra of the rare gases. In the case of potassium,[27] it has been shown that in addition to its arc spectrum it can, under moderate excitation, emit a series spectrum identical in type with that of the red spectrum of argon, and under violent excitation a spectrum having all the characteristics of the blue spectrum of this inert gas. In the case of the alkaline earths, the series spark spectra have the same characteristics as the arc spectra of the alkali elements.

But perhaps the most striking confirmation of the correctness of Bohr's view of the process of binding electrons to nuclei, and also at the same time of the validity of the Kossel-Sommerfeld Law, is found in recent work by Paschen[28] and by Fowler[29] on the spectra of doubly ionised aluminium and trebly ionised silicon.

It will be recalled that Fowler some years ago showed that the wave-lengths of the spark spectrum of magnesium could be organised into series having 4K for their Rydberg constant. Early this year Paschen carried the matter farther by showing that under strong excitation aluminium emits a spectrum that can be arranged into series with a Rydberg constant equal to 9K. Now Fowler has capped it all by showing in a brilliant piece of work that in the spark spectrum of silicon certain wave-lengths can be grouped into series with a Rydberg constant of 16K. With both elements the series referred to are doublet series of the type obtained in the arc spectrum of sodium.

[27] McLennan, *Proc. Roy. Soc.*, vol. 100, p. 182, 1921, and Zeeman and Dik, *Konin. Akad. Van Weten, Amsterdam, Proc.*, vol. xxv., p. 1, April 29, 1922, and *Ann. der Phys.*, Bd. 71, Heft 1/4, p. 188, 1923.

[28] Paschen, *Ann. der Phys.*, Bd. 71, Heft 1/4, p. 142, Heft 8, p. 537, 1923.

[29] Fowler, *Proc. Roy. Soc.*, vol. 103, No. A, 722, June 1923.

In terms of Bohr's theory the 9-fold value of the Rydberg constant would be interpreted as meaning that aluminium atoms which emitted this spectrum had lost two electrons, and were represented by Al++, or, as it is now written, Al(III). The 16-fold Rydberg constant would, on the same theory, also be interpreted as meaning that the atoms of silicon which emitted this spectrum were those that had lost three electrons, *i.e.* Si (IV). These results, it will be seen, amply confirm the view that the bound electrons in the neutral atoms of sodium, Na(I), are of the same type and are characterised by the same quantum numbers as those of the singly ionised atom of magnesium, Mg(II), of the doubly-ionised atom of aluminium, Al(III), and of the trebly-ionised atom of silicon, Si(IV)

What has been found to be true of the spectra of sodium, magnesium, aluminium, and silicon, will no doubt be found to be true of the spectra of the elements lithium, beryllium, boron, and carbon. The spectra of beryllium and boron are extremely meagre in wave-lengths, and but little is known of their spectral series. The spectrum of carbon, however, especially in the extreme ultra-violet, has been well worked out by a number of observers, and particularly so by Simeon.[30]

In the spectrum of beryllium the doublet $\lambda = 3131.194$ A, $\lambda = 3130.546$ A has been shown to be the first member of a principal and a second subordinate series of doublets. Moreover, Back,[31] who recently investigated its magnetic resolution, has found that the magnetic components are of the D_1 and D_2 type, just as Kent has shown the magnetic components of the close lithium doublet $\lambda = 6708$ A to be. It will, therefore, probably be found when the spectrum of beryllium has been extended that the doublet $\lambda = 3131.194$ A, $\lambda = 3130.546$ A will prove to be the first member of the doublet series of the positive singly-charged atom of beryllium, with a Rydberg constant for the series of 4K. In the spectrum of boron the doublets $\lambda = 2497.73$ A, $\lambda = 2496.78$ A and $\lambda = 2089.49$ A, $\lambda = 2088.84$ A, particularly the latter, merit attention in looking for a 9K series. In the ultra-violet spectrum of carbon there is a strong doublet at $\lambda = 1335.66$ A, $\lambda = 1334.44$ A, and another nearly as strong at $\lambda = 1329.60$ A, $\lambda = 1329.07$ A. These two also merit attention in any attempt to identify 16K series for this element.

In considering the general validity of the Kossel-Sommerfeld Displacement Law the recent work of Catalan [32] on the series spectra of manganese, chromium and molybdenum is of interest.

The spectra of the neutral and singly ionised atoms of manganese, as well as that of the neutral atoms of chromium, have been shown by him to consist of sets of sharp diffuse and principal triplet series. Moreover, he has found that in all these spectra there are certain groups of prominent lines, to which the name 'multiplet' has been given, that have similar characteristics, and that show similar variations with changes in temperature. This has led Catalan to put forward

[30] Simeon, *Proc. Roy. Soc.*, A, vol. 102, p. 490, 1923.
[31] Back, *Ann. der Phys.*, No. 5, p. 333, 1923.
[32] Catalan, *Phil. Trans.*, *Roy. Soc.*, Series A, vol. 223, pp. 127-173, 1922; *C.R.*, Jan. 8 and 22, and April 16, 1923.

the view that the neutral atom of manganese has an outer system of two electrons, and that when this atom loses one of its most loosely bound ones another electron from the next inner system comes out to take its place in the outermost system, so that the latter again contains two electrons. The similarity of the spectra of the neutral and singly ionised atoms of manganese would thus be accounted for. By assuming that this final configuration of the orbits in the singly ionised atoms of manganese was the same as the configuration of the outer orbits in the neutral atoms of chromium, the similarity of the arc spectrum of chromium to those of the singly ionised and neutral atoms of manganese would also be explained.

Catalan's series relations show that the two last acquired electrons in the neutral atoms of manganese and chromium are bound in equivalent orbits of the 4_1 type, and that as a consequence the ionisation potentials of these two elements are given by a frequency of the form $\nu = (1.5)$, and have the values 7.4 v. and 6.7 v. respectively.

In some later work Catalan [33] has shown that the scheme of series in the spectrum of molybdenum is identical with that which applies to the spectrum of chromium. With this element he deduced the value 7.1 volts for the ionisation potential. The two last acquired electrons in the atom of molybdenum would appear to be bound in equivalent orbits of the 5_1 type.

From the considerations that have been presented in regard to the atoms and the spectra of chromium and manganese some deductions can be made regarding the spectrum and stationary orbits of the unknown element of atomic number 43. Its arc spectrum, and probably that of its singly charged positive ion too, will likely consist of triplet series. Its spectrum will also very likely include a set of multiplets, and its two outer electrons will probably be found in equivalent 5_1 orbits. Although some considerations recently put forward by Lande [34] and by Back[35] may lead to modifications in the views expressed above, the possibility of making these deductions constitutes a rather remarkable testimony to the power of the methods that are being at present applied in unravelling the mysteries of atomic structure and of the origin of radiations.

An interesting point in connection with the Kossel-Sommerfeld Displacement Law arises in connection with the magnetic properties of the neutral atoms of argon, of the singly-charged positive atom ions of potassium, and of the singly-charged negative atom ions of chlorine. Recent work by W. L. Bragg [36] and Davey,[37] as well as a report by Herzfeld,[38] go to show that the ions referred to and the atom of argon have practically the same dimensions, with a radius of about 1.56×10^{-8} cm. It appears also from the work of Königsberger,[39]

[33] Catalan, *C.R.*, April 16, 1923.
[34] Lande, *Zeit. für Phys.*, vol. 15, p. 189, 1923.
[35] Back, *Zeit. für Phys.*, vol. 15, p. 206, 1923.
[36] W. L. Bragg, *Phil. Mag.*, vol. xl., p. 187, 1920.
[37] Davey, *Phys. Rev.*, vol. xviii., p. 103, 1921.
[38] Herzfeld, *Jahr. der Rad. und Elek.*, Bd. 19, p. 259, 1922.
[39] Königsberger, *Ann. der Phys.*, vol. 66, p. 713, 1898.

St. Meyer,[40] Curie,[41] and Soné [42] that the atom ions of potassium and chlorine and the atoms of argon are diamagnetic.

Since these ions and the atoms of argon contain the same number of electrons, and since the electrons in all three are supposed to be bound in orbits of the same type and of the same area, one would expect them to show identical diamagnetic properties. The experimental results, however, do not appear to support this view. While the specific magnetic susceptibility of argon has been shown by Soné to have the value 5.86×10^{-6}, that of the singly-charged negative atom ions of chlorine and of the singly-charged positive atom ions of potassium have been found from observations on the magnetic properties of potassium chloride to be equal approximately to 0.55×10^{-6}, *i.e.* the diamagnetic susceptibility of argon is about ten times that of the ions of potassium and chlorine. As Pauli [43] has shown that this high value of the diamagnetic susceptibility of argon leads on certain simple assumptions to a value for the moment of inertia of the atoms of argon about ten times too great, it would appear that the discrepancy arises in connection with the evaluation of the diamagnetic susceptibility of argon. Although all the experimental work involved appears to have been carefully done, it is evident that the investigation of the diamagnetic properties of these elements will have to be carried further before the matter is finally cleared up.

Quantisation in Space.

One of the most surprising and interesting developments of the quantum theory is that which shows that quantum numbers determine not only the size and form of the electronic Keplerian orbits in atoms, but also the orientation of these orbits in space with regard to a favoured direction such as that provided by an intra-atomic or by an external magnetic or electric field of force. For any arbitrary value of the azimuthal quantum number k, the simple theory shows that there are exactly $k+1$ quantum positions of the orbital plane characterised by whole numbers. For example, if $k=1$ the normal to the orbit may be either parallel to the direction of the controlling field or at right angles to it. If $k=2$ the normal to the orbit may take up in addition to these two positions a third one, in which the normal to the orbit makes an angle of 60° with the field. For higher values of the quantum number k, the possible orientations of the corresponding orbits become regularly more numerous.

A striking confirmation of this theory is afforded by the very beautiful experiments of Gerlach and Stern.[44] In these a stream of atoms of vaporised silver was allowed to flow past a wedge-shaped pole of an electromagnet which provided a radial non-uniform magnetic

[40] St. Meyer, *Ann. der Phys.*, vol. 69, p. 239, 1899.

[41] Curie, *Jl. de Phys.*, 3, S. 4, p. 204, 1895.

[42] Soné, *Tôhoku Univ. Sc. Rep.*, vol. 8, pp. 115-167, Dec. 1919, and *Phil. Mag.*, vol. 39, p. 305, March 1920.

[43] Pauli, *Zeit. für Phys.*, Bd. Heft 2, p. 201, 1920.

[44] Gerlach and Stern, *Zeit. für Phys.*, vol. 7, p. 249, 1921; vol. 8, p. 110, 1921; vol. 9, p. 349 and p. 353, 1922.

field. The atoms were caught on a glass plate placed immediately behind the pole, and it was found that they were deposited in two distinct sharply defined layers, indicating that the atoms were sorted out into two distinct and separate beams. The positions of the bands on the plate showed that one of the beams was attracted by the pole and the other repelled by it, the attraction being slightly the greater in intensity. No evidence was obtained of an undeflected beam. From these results it was concluded that all the silver atoms in the stream of vapour possessed a definite magnetic moment, and that while the atoms were passing through the magnetic field their magnetic axes had two distinct orientations in space.

By assuming the correctness of this interpretation, Gerlach and Stern found from measurements on the various magnitudes involved in the phenomenon that within the limits of error of their experiments the magnetic moment of the normal atom of silver in the gaseous state was that of one Bohr magneton.

Bohr, also, has drawn attention to another possible illustration of the principle of the quantisation of orbits in space. It is known that all the rare gases do not exhibit the property of paramagnetism. From this fact the conclusion has been drawn that the atoms of these gases in their normal condition do not possess any angular momentum. According to the quantum theory, however, this conclusion may not be warranted, for we have seen that for an atom which has a finite angular momentum and, consequently, possesses a magnetic moment, the theory prescribes certain definite directions for the axis of momentum relative to a magnetic field in which the atom may be situated. If we assume that the atoms of the rare gases in a magnetic field can place themselves with their momentum axes perpendicular to the magnetic field, it follows that they could appear to be diamagnetic, and all indication of paramagnetism on their part would be absent. In this connection I may point out that Bohr has made the suggestion that evidence in support of the validity of this view is derivable from the results of an analysis, on the basis of the quantum theory, of the anomalous Zeeman effect shown by the rare gases.

One point that may be worthy of notice in dealing with phenomena associated with the principle of space quantisation is that the permitted orientations depend only on the values of the quantum number involved, and not on the magnitude of the magnetic field applied.

Orbits characterised by certain definite values of the quantum number should take up their permitted orientations in weak magnetic fields as well as in strong ones, provided the time allowed for the process to take place was ample, and provided suitable pressures were used and disturbances arising from the presence of contaminating gases were eliminated. Such conditions as these have recently been realised by Gerlach and Schutz,[45] and they have been able to obtain with sodium vapour at low pressures in the absence of foreign gases remarkably striking manifestations of the magnetic rotation of the plane of polari-

Gerlach and Schutz, *Die Naturwissenschaften*, vol. 11, Heft 28, p. 638, 1923.

sation of the light passing through the vapour with magnetic fields as low as a few tenths of a gauss.

This idea of space quantisation may perhaps throw some light on the interesting and suggestive experiments of R. W. Wood and A. Ellett [46] on the polarisation of the resonance light emitted by mercury and sodium vapours. In their experiments, it will be recalled, strong polarisation of the resonance light from mercury or sodium vapours could be produced by weak magnetic fields properly orientated. Moreover, they found that the polarisation of the resonance light emitted by these vapours in the presence of the earth's magnetic field could be destroyed by applying a magnetic field of less than one gauss provided it was suitably orientated. It is highly desirable that the experiments of Wood and Ellett should be followed up in order that sufficient information may be gained to enable us to elucidate the principles underlying the modifications in the polarisation of the resonance light observed by them.

It seems clear that atoms of sodium, for example, when excited by the absorption of resonance radiation would tend during the period of excitation to take up definite and characteristic orientations even in weak magnetic fields that would result in the polarisation of the resonance radiation emitted being different from that of the radiation emitted from atoms of the vapour situated in space in which absolutely no magnetic field existed. It should be remembered, too, that in the normal atom of sodium the orbit in which the valency electron is bound has the value 1 for its characteristic azimuthal quantum number k. When the atom is excited by the absorption of resonance radiation the azimuthal quantum number of the orbit, in which the valency electron becomes bound for a time, takes on the value 2. It seems clear then that the electronic orbit of the valency electron may be subject to different orientations relative to the rest of the atom when the atom is in the excited state from what it would be with the atom in its normal state. These relative orientations, moreover, would again be different in the presence of even a weak external magnetic field from what they would be in the complete absence of such a field. It is, therefore, quite conceivable that changes in orientation of electron orbits may be able to account for the phenomena observed by Wood and Ellett, but at present the whole matter appears to be rather involved and rather difficult to clear up with the information as yet available.

Quantum Theory and the Zeeman Effect.

Among the most fruitful of the principles utilised by Bohr in the development of his theory of radiation is the Adiabatic Hypothesis enunciated by Ehrenfest.[47] To this hypothesis Bohr has given the name the Principle of Mechanical Transformability. Numerous examples of the application of this principle might be cited, but the one that concerns us most here is that which deals with the effect of the establishment of a magnetic field on the electronic orbits in atoms. It

46 Wood and Ellett, *Proc. Roy. Soc.*, A, June 1923, p. 396.

47 Ehrenfest, *Die Naturwissenschaften*, vol. 11, Heft 27, July 6, 1923, p. 543.

is well known that Larmor has shown that one result of the establishment of such a field is to endow an electronic orbit with a uniform rotation about the direction of the magnetic field, the angular velocity being given by $\omega = \frac{1}{2} \frac{e}{m} \frac{H}{c}$. Langevin has also pointed out that the size and form of the electronic orbit remain unaffected by the magnetic field. Ehrenfest's hypothesis asserts that if the magnetic field be established slowly the energy of the electron in its orbital motion and the frequency of its revolution in the orbit may be changed, but the *number* of quanta defining its energy undergoes no modification. With the adoption of these principles it is an easy matter to show that when we quantise the angular momentum about the direction of the magnetic field the normal Zeeman components are exactly the same as those provided by the older classical theory of Lorentz. The singular beauty and simplicity of this method of explaining the normal Zeeman effect constitute one of the finest achievements placed to the credit of the quantum theory

Efforts to explain the abnormal Zeeman effect have not as yet met with the same success. Among the contributions made to this subject perhaps that of Heisenberg[48] is the most stimulating and suggestive. In addition to offering an explanation of the abnormal Zeeman effect it constitutes an attempt to account for the doublet and triplet structure of series spectra.

Taking for example the case of an alkali element, Heisenberg postulates that through magnetic coupling a movement of rotation within an atom of these elements involves simultaneously the valency electron and the core of the atom as well. According to the theory it is supposed that in the various stationary states there is a partition of the angular momentum between the two, one-half an azimuthal quantum being assigned to the core and $k-\frac{1}{2}$ azimuthal quanta to the electron. The author supposes further that through space quantisation the two axes of rotation are in the same direction, and that the rotation of the core and that of the electron may take place either in the same sense or in opposite senses. As far as the radial quanta for the electronic orbits are concerned, it is assumed that they are given by $n'+\frac{1}{2}$ where n' has integral values. This device leads to the result that the total quantum number characterising the orbit of the electron is an integer n that is equal to the sum $k+n'$. In this way the author is enabled, at the same time, to characterise the spectral terms in the Rydberg series formulæ by integral quantum numbers.

This scheme, it will be noted, provides for the binding of the valency electron in one or other of two energy levels and reduces the frequency difference characterising the members of the doublet series of the spectra of the alkali elements to a manifestation of what is practically a Zeeman effect produced by an internal atomic magnetic field. To account for the triplet structure of series spectra such as we obtain with the alkaline earth elements, Heisenberg supposes the magnetic coupling

48 Heisenberg, *Zeit. für Phys.*, No. 8, p. 257 and p. 273, 1922.

to involve not only the core of the atom but the two outer valency electrons as well. It is shown when the theory is extended to take account of an external magnetic field in addition to the internal one, that the Zeeman separations of the magnetic components of doublet and triplet lines are in exact agreement with the laws formulated by Preston and Runge.

When the external magnetic field is high compared with the internal one, the theory shows that for doublets and triplets the final result is a normal Zeeman triplet in complete accordance with the observations of Paschen and Back.[49]

To illustrate the validity of the theory Heisenberg used his formulæ to evaluate the magnitude of the internal magnetic field of the atoms of lithium, and found that it led to a value of 0.32cm^{-1} for the frequency difference characterising the doublets of the second subordinate series in the spectrum of this element. As the experimental value found by Kent[50] is 0.34cm^{-1}, it will be seen that the agreement is good.

Again, in connection with the matter of triplet series the theory shows that in the case of the *p* terms the ratio of the triplet frequency differences should be as 2 : 1, for the *d* terms it should be as 3 : 2, and for the *f* terms as 4 : 3. These deductions find ample verification in the measurements made on the frequency differences of triplet series in the spectra of such elements as magnesium, calcium, strontium, barium, zinc and cadmium.

To say the least, the theory outlined above is extremely suggestive. It leads, however, to rather surprising results. If we are to account for doublet separations generally as being due to Zeeman separations produced by intra-atomic magnetic fields, it follows that with some atoms these must be exceedingly high. Taking the doublet separations of the second subordinate series in the spectra of the alkali elements, we find the following values for the internal magnetic fields of the different atoms :—

Element	$\Delta\nu_p$	H_i
Lithium	0·34 cm^{-1}	7,173 Gauss
Sodium	17·18 „	366,744 „
Potassium	57·71 „	1,231,945 „
Rubidium	237·6 „	5,072,090 „
Cæsium	554·0 „	11,826,330 „

If it should turn out that magnetic fields so high as those given above are present in atoms of elements such as those in the alkali group, the results obtained by Wood and Ellett would be easily explained.

Whether the existence of a magnetic coupling between the valency electron and the atomic core justifies Heisenberg in adopting the artifice of partitioning the quanta of rotation between the electron and the atomic core is a debatable point.

It does not appear to be permissible to adopt the value ½ for the azimuthal quantum number in defining the stationary orbits of a heavy atom such as that of uranium. In a recent paper by Rosseland,[51] in

[49] Paschen and Back, *Ann. der Phys.*, vol. 39, p. 897, 1912; vol. 40, p. 960, 1913.

[50] Kent, *Ast. Phys. Jl.*, vol. 40, p. 343, 1914.

[51] Rosseland, *Nature*, March 17, p. 357, 1923.

which a suggestion is put forward that the phenomenon of radioactivity exhibited by the heavier atoms may be due to some interaction between the nuclear and the external electrons in these atoms, he finds that the nearest approach of an electron to the nucleus in the atom of uranium according to Bohr's scheme of orbits is 16×10^{-12} cm. If the electronic orbit closest to the nucleus in the atom of uranium had $\frac{1}{2}$ for the value of its azimuthal quantum number, it would mean that the shortest distance of approach to the nucleus would be equal to 4×10^{-12} cm. As the radius of the nucleus of the atom of uranium has been shown to be 6.5×10^{-12} cm. it is evident that such an orbit could not exist. For reasons of this character we are practically precluded from assigning to k, the azimuthal quantum number, a value less than 1 in defining the electronic orbits in atoms.

In this paper an attempt has been made to outline some of the leading features of the quantum theory as it is being used to solve the problems of atomic structure as well as of those connected with the origin of radiations emitted by atoms. Other illustrations of special interest might have been drawn from the treatment of problems that have arisen in a study of band spectra[52] and of fluorescence phenomena.[53] The recent work of Cabrera,[54] Epstein,[55] and Dauvillier,[56] on paramagnetism, too, has a most interesting connection with the development of inner systems of electronic orbits in atoms in Bohr's scheme of the genesis of atoms.

I venture to think, however, that the few illustrations presented may serve, in a measure, to indicate the power and also the beauty of the methods being put forward to elucidate the problem of the origin of radiation.

[52] Kratzer, *Die Naturwissenschaften*, vol. 11, Heft 27, p. 577, 1923.

[53] Franck and Pringsheim, *Die Naturwissenschaften*, Heft 27, vol. 11, July 6, p. 559, 1923.

[54] Cabrera, *Jl. de Phys.*, t. 6, p. 443, 1922.

[55] Epstein, *Science*, vol. lvii., No. 1479, p. 532, 1923.

[56] Dauvillier, *C.R.*, June 18, p. 1802, 1923.

SECTION B.—CHEMISTRY.

SOME ASPECTS OF THE PHYSICAL CHEMISTRY OF INTERFACES.

ADDRESS BY

PROFESSOR F. G. DONNAN, C.B.E., F.R.S.,

PRESIDENT OF THE SECTION.

It was at the last meeting at Liverpool, in 1896, that I first had the honour of attending a gathering of the British Association. On that occasion Dr. Ludwig Mond, F.R.S., was President of Section B, and I shall never forget the interest and pleasure I felt in listening to the Address of that great master of science and scientific method. Little did I dream that in 1923 I should have the honour and privilege of occupying the Chair of Section B at Liverpool.

Looking back on the Liverpool Meeting of 1896, one can say now that it came at the dawn of a new era in the development of physico-chemical science. The X-rays had just been discovered by Röntgen. Perrin had proved experimentally (1895) that a negative electric charge was associated with the cathode rays and had surmised that these so-called 'rays' were constituted by electricity in motion, thus corroborating Crookes' brilliant views of a decade earlier and demonstrating that Lenard was wrong. Sir J. J. Thomson had just begun that splendid series of researches which resulted not only in the complete elucidation of the nature of the cathode 'rays,' but also in the discovery of the negative electron as a constant, universal, and fundamental constituent of all matter.

The discovery of the chemically inert elementary gases by Rayleigh and Ramsay had begun in 1894, and the series of investigations which finally led to the recognition of the radio-active transformations of atoms and to the discovery of the nature and constitution of the atom itself, were just beginning. During the last twenty-five years the influence of these discoveries on chemical science has been enormous. There has come about a fresh reunion of physics and chemistry, somewhat analogous to that which occurred in the days of Volta and Davy. During the two decades preceding 1896, physical science had been largely concerned with the phenomena of the 'ether,' with electric and magnetic fields, electromagnetic waves, and the identification of light and other

forms of radiant energy as electromagnetic phenomena. Now that the physicists have brought physical science back to the close and intimate study of matter, physics and chemistry have come together again, and the old and homogeneous science of 'natural philosophy' has been reconstituted It is time that the walls which divide our chemical and physical laboratories were broken down, and that the young men and women who come to our Universities to study physics *or* chemistry, should study the facts and principles of a fundamental science which includes both.

Since the last meeting of our Section a number of eminent men of science have passed away. It is with great sorrow that I record the deaths of Professor Sir James Dewar, F.R.S., in our own country, and of Professors E. Beckmann, J. P. Kuenen, G. Lemoine, L. Vignon, and J. D. van der Waals on the Continent. Limits of time and space forbid me to attempt here any account of the great services to science rendered by these eminent men. As the successor of Tyndall, Sir James Dewar worked for over forty years at the Royal Institution, and by his investigations on the liquefaction of gases and the physical and chemicial behaviour of substances at low temperatures, upheld the famous tradition of the Royal Institution as a home of pioneer research in science. Beckmann's name is well known for his researches on the effect of dissolved substances on the boiling- and freezing-points of solvents, and for the convenient form which he gave to the 'variable zero' thermometer. He also devised useful and convenient forms of apparatus required in spectroscopic work. Lemoine was one of the pioneers in the study of chemical reaction velocities and equilibria in France, whilst Vignon was well known for his researches in organic chemistry. Kuenen was at one time Professor of Physics at Dundee, although at the time of his death he had been for many years one of the Professors of Physics at Leiden. He was particularly noted for his investigations on the equilibria occurring in the evaporation and condensation of liquid mixtures. His death at a comparatively early age is a very heavy loss to science in general, and to Holland in particular. In van der Waals there passes away one of the very greatest men of science. He was one of that group of Dutch men of science, including Cohen, Lorentz, Kamerlingh Onnes, van't Hoff, Roozeboom, Schreinemakers, and Zeeman, who have made Holland so famous as a centre of physical and chemical research during the last thirty or forty years. Van der Waals was the great mathematical and physical interpreter of the work begun by Thomas Andrews.

In recent years a great deal of attention has been paid by chemists, physicists and physiologists to the phenomena which occur at the surfaces or interfaces which separate different sorts of matter in bulk. During the last quarter of the nineteenth century, both J. Willard Gibbs and J. J. Thomson had shown clearly, though in different ways, the peculiar nature of these interfacial or transitional layers. It was evident that things could happen in these regions which did not occur in the more homogeneous and uniform regions well inside the volume of matter in bulk. Such happenings might, if they could be investigated, reveal molecular or atomic peculiarities which would be undetect-

able in the jostling throng of individuals inside. A surface or surface layer represents a sort of thin cross section which can be probed and examined much more readily than any part of the inside bulk. It is indeed only within comparatively recent years that the X-rays have provided a sufficiently fine probe for examining this bulk in the case of crystalline matter.

The living organisms of plants and animals are full of surfaces and membranes. What can happen at surfaces is therefore a matter of great importance for the science of living things. We are bound to hold as long as possible to the assumption that the physico-chemical manifestations of life can be explained in terms of the potentialities of action inherent in electrons, atoms, and molecules. The drilled and disciplined soldiers of an army behave very differently from an undisciplined and disordered mob of the same men. Thus the modes of action of ordered arrays and marshallings of atoms and molecules are of extreme interest, since such modes of action will constitute phenomena non-existent in a disordered multitude of the same atoms and molecules with exactly the same individual powers and potentialities. These phenomena may be intimately connected with the phenomena of living matter, and as the latter evidently require the existence of surfaces and membranes, the idea naturally suggests itself that the special arraying or ordering of individuals occurs at, and may start from, such surfaces.

An essential characteristic of this ordering or arraying may consist in *special orientation.* In the chemical and physical actions occurring in a volume of liquid whose bulk is large compared with its surface, the molecules or atoms probably move towards each other with every sort of orientation, no special type being privileged or distinguished. Should, however, some special orientation be characteristic of interfaces, then it is clear that such interfaces will exhibit new phenomena due to this special sort of arraying. Moreover, if we are dealing with molecules which are ionised into electrically polar constituents, or which, if not actually dissociated, can be treated as electrically bi-polar, it follows that, if orientation occurs at interfaces and surfaces, then electrical double layers and electrical potential differences may be set up at such boundaries.

In the theories of Laplace, Gauss, and Poisson the field of force surrounding an attracting element or molecule was regarded as essentially uniform in its spatial relations, *i.e.* the equipotential surfaces were regarded as concentric spheres with the molecule as a small element at the centre. The only way in which the molecule could show its character was in affecting the intensity of this central force at a given distance and the rate at which the force falls off with increasing distance. The molecules were thought of as possessing what one might call a very rounded and somewhat monotonous 'physical' personality or character as regards their fields of force. In recent years our views on such matters have undergone a somewhat radical transformation. The field of force surrounding a molecule may in reality be very 'irregular,' and may be specially localised around certain active or 'polar' groups. Its region of sensible magnitude may be very variable

and relatively small compared with molecular dimensions. The chemical constitution of the molecule is now regarded as determining the varying nature of the field of force surrounding it, so that parts of the molecule possessing high 'residual chemical affinity' give rise to specially powerful regions of force. In this way the older 'physical' theories of cohesion according to central forces with uniform orientation have been to some extent replaced, or at all events supplemented, by 'chemical' theories according to which the attractive force-fields are highly localised round active chemical groups and atoms, are relatively minute in range, and can be saturated or 'neutralised' by the atoms or groups of neighbouring or juxtaposed molecules.

Dr. W. B. Hardy has been the chief pioneer in the development of these newer theories, having been led thereto by his researches on surface tension, surface films, composite liquid surfaces and static friction and lubrication. As the matter is one of great importance, I shall take the liberty of giving two quotations from Hardy's scientific papers.

'The corpuscular theory of matter traces all material forces to the attraction or repulsion of foci of strain of two opposite types. All systems of these foci which have been considered would possess an unsymmetrical stray field—equipotential surfaces would not be disposed about the system in concentric shells. If the stray field of a molecule, that is of a complex of these atomic systems, be unsymmetrical, the surface layer of fluids and solids, which are close-packed states of matter, must differ from the interior mass in the orientation of the axes of the fields with respect to the normal to the surface, and so form a skin on the surface of a pure substance having all the molecules oriented in the same way instead of purely in random ways. The result would be the polarisation of the surface, and the surface of two different fluids would attract or repel one another according to the sign of their surfaces.' (Hardy, 1912).

These ideas are even more clearly expressed in the following passage. 'If the field of force about a molecule be not symmetrical, that is to say, if the equipotential surfaces do not form spheres about the centre of mass, the arrangement of the molecules of a pure fluid must be different at the surface from the purely random distribution which obtains on the average in the interior. The inwardly directed attractive force along the normal to the surface will orientate the molecules there. The surface film must therefore have a characteristic molecular architecture, and the condition of minimal potential involves two terms—one relating to the variation in density, the other to the orientation of the fields of force.' (Hardy, 1913).

Hardy thus bases the notion of molecular orientation at the surface on the existence of unsymmetrical fields of force surrounding the molecule; in other words, the parts of the molecule possessing the most powerful stray fields will be attracted inwards towards the bulk and thus cause a definite orientation of the whole molecule at the surface.

If γ_A be the surface tension of a liquid A, γ_B that of another practically immiscible liquid B, and γ_{AB} the interfacial tension at the interface A/B, then the quantity $W = \gamma_A + \gamma_B - \gamma_{AB}$ represents the decrease of free surface energy, and therefore the maximum work gained,

when a surface of A is allowed to approach normally and touch a surface of B at constant temperature. Comparing different liquids A with water as a constant liquid B, Hardy has shown that the quantity W is extremely dependent on the chemical constitution of A, and is especially high when A contains the atomic groups characteristic of alcohols, acids, and esters. Thus, for such saturated substances as octane, cyclohexane, CS_2 and CCl_4, the values of W at ordinary room temperature lie between 21 and 24. Compare with these values the following:—

(*a*) Introduction of a hydroxyl group:—

Octyl Alcohol	46
Cyclohexanol	51.4

(*b*) Introduction of a carboxyl group:—

n—Caprylic acid . . .	46.4
Oleic acid	44.7

The natural inference from results such as these is that the cohesional forces are essentially chemical in origin and that they depend in large measure on the presence of 'active' atoms or groups of atoms, namely those possessing strong fields of 'residual chemical affinity'; in other words, powerful and highly localised stray fields of electrical or electromagnetic force (or of both types). The existence of such atoms or atomic groups is strong presumptive evidence of the unsymmetrical fields of force postulated by Hardy and therefore of the molecular orientation at surfaces.

The conclusions drawn by Hardy have been amply confirmed by W. D. Harkins, and his collaborators, who in a long series of accurate measurements of surface and interfacial tensions have found that in the case of very many organic liquids the 'adhesional work' towards water is greatly increased by the presence of oxygen atoms (as in alcohols, acids, and aldehydes). They find that the very symmetrical halogen derivatives CCl_4 and $C_2H_4Br_2$ (which possess specially high values for their own cohesional work) give markedly low values for their adhesional work towards water, and that in the case of unsymmetrical molecules, the adhesional work towards water is determined by the presence of certain active atoms or atomic groups.

In his work on static friction and lubrication, Hardy has found that the influence of chemical constitution and the effects of active atomic groups are very pronounced. This, comparing aliphatic or open chain compounds, the co-efficient of static friction falls (and the lubricating power increases) as we pass through the series hydrocarbon—alcohol—acid. The corresponding ester is in this case a much worse lubricant than the related acid or alcohol. These results suggest, as Hardy has indicated, that friction is caused by the molecular cohesion of surfaces, and that in the action of such lubricants the molecules are oriented with their long axes normal to the surface, whereby the active atomic groups play an important part in 'taking up' or saturating a portion of the stray force-fields of the molecules of the solid surfaces, and in orienting and anchoring the lubricant molecules to these surfaces. Many facts lend strong support to Hardy's views. Thus it is true, I believe, that the addition of aliphatic esters improves the lubricating

value of hydrocarbon oils, whilst H. Wells and W. Southcomb have demonstrated the marked improvement due to a small addition of a fatty acid. In this connection it is interesting to note that W. E. Garner and S. S. Bhatnagar have recently shown in my laboratory that the interfacial tension between mercury and B.P. paraffin oil is markedly lowered by small additions of oleic acid. The oleic acid molecules are therefore absorbed or concentrated at the mercury-oil interface, an action which may well be due in part to the fixation and orientation of these molecules at the metal-oil interface.

This question of the orientation of molecules at the surfaces of liquids has been greatly extended in recent years by a detailed study of the extremely thin and invisible films formed by the primary spreading of oily substances on the surface of water. In a continuation and development of the work of Miss Pockels, the late Lord Rayleigh showed many years ago that when olive oil forms one of these invisible films on water, there is no fall in surface tension until the surface concentration reaches a certain very small value. He made the highly interesting and important suggestion that this concentration marks the point where there is formed a continuous layer just one molecule thick. In the case of olive oil, he found this critical thickness to be 10^{-7} cm., and concluded that this number represented the order of magnitude of the diameter of a molecule of the oil. Increase in surface concentration beyond this point causes the surface tension to fall, until a second point is reached, after which no further fall in surface tension occurs. Lord Rayleigh assumed that at the second point a layer two molecules thick is formed. This pioneer work of Lord Rayleigh was repeated and extended by H. Devaux and A. Marcelin, who showed the correctness of his first suggestion, namely that the primary film consists of a *unimolecular* layer. It appears, however, that the fall in surface tension which he ascribed to the building up of a bimolecular layer, must be ascribed to the closer packing of the molecules of the unimolecular layer, and that the second point occurs when these molecules are packed as tightly as possible.

Instead of varying the surface concentration by adding more and more of the oily substance to a definite surface, we may attain the same end by means of a moving boundary and a variable surface, and study the relation between the force of surface-compression (difference between the surface tension of pure water and that of the contaminated surface) and the surface concentration. This method was greatly developed by Devaux. Although these researches had firmly established the theory of the formation of a unimolecular surface layer and therefore of the existence of a *new 'two-dimensional' phase of matters*, we owe it to I. Langmuir to have made a very important advance by connecting this conception with the ideas of chemically active groups and molecular orientation. Influenced, no doubt, by the ideas of Hardy, Langmuir reasoned that the formation of these primary unimolecular films must be due to the presence of active groups in the molecules, which are attracted inwards towards the water and thus cause the long open chain molecules of the fatty acids to be oriented on the water surface with their long hydrocarbon axes vertical and side by side.

Working by means of the method of Devaux, Langmuir put these ideas to the test of experiment, and determined the internal molecular dimensions of a unimolecular layer. The following is an excerpt from the results which he published in 1917:—

—	Molecular Cross Section (S) (sq. cms.)	$\sqrt{S}$	Molecular Length (cms.)	Length per C atom (cms.)
Palmitic Acid . . .	21×10^{-16}	$4{\cdot}6\times10^{-8}$	$24{\cdot}0\times10^{-8}$	$1{\cdot}5\times10^{-8}$
Stearic Acid . . .	22×10^{-16}	$4{\cdot}7\times10^{-8}$	$25{\cdot}0\times10^{-8}$	$1{\cdot}4\times10^{-8}$
Cerotic Acid . . .	25×10^{-16}	$5{\cdot}0\times10^{-8}$	$31{\cdot}0\times10^{-8}$	$1{\cdot}2\times10^{-8}$

It is at once evident that these results agree in a wonderful manner both with the idea of a unimolecular layer and with that of molecular orientation. The molecular cross section is practically constant, as we should expect, since it must represent the cross section either of a carboxyl or CH_3 group. Since the molecular length is determined from the thickness of the layer, and is found to be five or six times the value of $\sqrt{S}$ (molecular 'thickness'), we perceive here the first actual experimental proof of the theory of molecular orientation. Another fact of great significance emerges from these results. If we calculate the average distance between two adjacent carbon atoms in the three acids, we obtain a value of 1.4×10^{-8} cm. Now this distance must be of the order of magnitude of the distance between the centres of the carbon atoms in the crystal structure of a diamond. This latter distance is known to be 1.52×10^{-8} cm. The agreement is striking.

These regularly oriented and unimolecular surface films on water have been recently investigated in a very detailed and careful manner by N. K. Adam, who has improved the method employed by Devaux and Langmuir. From a closer analysis of the relationship between the force of surface compression and the surface concentration (expressed as area occupied per molecule) he has shown that a distinction must be made between the close packing of the polar or active end groups (head groups) of the molecules and the subsequent close packing of the hydrocarbon chains. The following table contains a few of Adam's results for the higher aliphatic acids:—

—	No. of C atoms	Cross Section (sq. cms.) $\times10^{16}$		Approx. Length (cms.) $\times10^{8}$
		Chain	Head	
Myristic Acid . .	14	21·0	25·1	21·1
Pentadecylic Acid . .	15	21·0	25·1	22·4
Stearic Acid . . .	18	21·0	25·1	26·2
Behenic Acid . .	22	21·0	25·1	31·4

Although these results must be considered as more accurate and detailed than those of Langmuir, they provide an ample confirmation

of the theory of unimolecular films of juxtaposed and oriented molecules. If we calculate the average distance between two carbon atoms for the four acids, we obtain the following results:—

	Distance (cms.) $\times 10^8$
Myristic Acid	1·6
Pentadecylic Acid	1·6
Stearic Acid	1·5
Behenic Acid	1·5

As pointed out before, these values do not deviate much from the value for the distance between the carbon atom centres in the diamond (1.52×10^{-8} cm.). Too much stress cannot, however, be laid on this point, since in calculating the lengths of the oriented carbon chains an assumption has to be made regarding the density of the film, because only its area and mass are given directly by experiment.

Concerning this point some very interesting results have been recently obtained in Sir William Bragg's laboratory by Dr. A. Müller. In these experiments layers of crystallised fatty acids on glass plates have been examined by an X-ray photographic method. From these results it appears that the unit cell is a long prism, the cross section of which remains constant for the substances investigated, whilst the length of the prism increases linearly with the number of carbon atoms in the molecule. The increase in length of the unit prism per carbon atom in the molecule is found to be 2.0×10^{-8} cm. Since it appears likely that there are *two* molecules arranged along the long axis of each unit cell (prism), it would follow that the increase in the length of the molecule per carbon atom added is 1.0×10^{-8} cm. Comparing this result with the value for the distance between the carbon centres in the diamond lattice, it would appear that the carbon atoms in the long hydrocarbon chains of the higher saturated fatty acids are arranged in a zig-zag, or more probably in a spiral or helix. If this be the case, the closer packing or compression of the juxtaposed molecules in the unimolecular films, as revealed in the investigations of Devaux, Langmuir, and Adam, may be to some extent explained by the straightening out of these zig-zags, or perhaps by the 'elastic compression' of the helices.

As pointed out by Langmuir, the question of the formation of unimolecular surface films can be attacked in a different manner. It is known that gases or vapours can be condensed or adsorbed by solid and liquid surfaces. The question then arises, does the formation of primary unimolecular films ever occur in such cases? It will be recollected that Hardy made the suggestion that the formation of the primary unimolecular film in the spreading of oily substances on water might be due to adsorption from the vapour. In order to examine this question, Mr. T. Iredale has recently measured in my laboratory the fall in the surface tension of mercury caused by exposing a fresh mercury surface to vapours of increasing partial pressure. The excess surface concentration q of the adsorbed vapour can then be calculated by means of Gibbs' formula

$$q = -\rho \frac{d\gamma}{dp}$$

where γ = surface tension, and ρ and p denote the density and partial pressure of the vapour respectively. Working with the vapour of methyl acetate, Iredale found in this way that at a temperature of 26° C. and a partial pressure of 62 mm. of mercury, $q = 4.5 \times 10^{-8}$ grm. per square centimetre of surface. From this result we can readily calculate that there are 0.37×10^{15} molecules of methyl acetate adsorbed per square cm., and that the area per molecule is 27×10^{-16} sq. cm. As under the conditions corresponding to this calculation the molecular surface layer was probably not quite saturated (in the unimolecular sense), we may expect the value found to be of the same order of magnitude but somewhat greater than the values found by Adam for the cross section of the head group of the higher saturated fatty acids (25×10^{-16}) and of the esters (22×10^{-16} for ethyl palmitate and ethyl behenate). We may, therefore, say that Iredale's results appear to indicate the formation of a primary unimolecular layer built up by adsorption from the vapour phase.

Langmuir has measured the adsorption of a number of gases at low temperatures and pressures on measured surfaces of mica and glass, and has arrived at the conclusion that the maximum quantities adsorbed are always somewhat less than the amounts to be expected in unimolecular surface layer. E. K. Carver, who has measured the adsorption of toluene vapour on known glass surfaces, has arrived at a similar conclusion. The view that the *maximum* adsorption from the gas phase cannot exceed a unimolecular layer has, however, been much criticised. Thus, for example, M. E. Evans and H. J. George, on the basis of their own measurements on the adsorption of gases on a known surface of glass wool, combined with the data obtained by Mülfarth, have concluded that the adsorption layer may be several (and in some cases many) molecules thick. It may well be that the formation of a unimolecular 'saturation' layer only occurs in the case of molecules with relatively very active atoms or atomic groups, whose strong localised fields of force suffice to produce powerful attraction and orientation and an almost complete saturation of the 'stray' fields of the surface molecules of the adsorbing surface, especially when the thermal temperature agitation is sufficiently small. In the case of molecules with weaker or more symmetrical fields of force, there may be relatively little orientation, and an extension of the attraction field of the adsorbent through layers of the adsorbate many molecules thick. It would be rash to theorise too much on this subject until more data are accumulated, but it may be pointed out that in his investigations on the spreading of surface films and on the theory of lubrication, Hardy has been led to distinguish between primary spreading (primary unimolecular films) and secondary spreading (secondary relatively thick sheets).

Let us now consider another type of formation of surface layers at the surfaces of liquids—namely, the case where a substance dissolved in a liquid concentrates preferentially at the liquid-air or liquid-vapour interface. Gibbs, and later J. J. Thomson, have shown that if a dissolved substance (in relatively dilute solution) lowers the surface tension, it will concentrate at the surface. That such a phenomenon actually occurs has been qualitatively demonstrated in the experiments of D. H. Hall, J. von Zawidski, and F. B. Kenrick and C. Benson,

by the analysis of foams and froths. In 1908 S. R. Milner used the same method in the case of aqueous solutions of sodium oleate, and arrived at a mean value of 1.2×10^{-10} gram mols. excess concentration per sq. cm. of surface. Now, in the case of dilute solution, we can calculate q, the amount concentrated or 'adsorbed' in the surface per sq. cm. (excess surface concentration) by making use of the equation of Gibbs,

$$q = -\frac{d\gamma}{d\mu}$$

where γ = surface tension and μ = chemical potential of the adsorbed substance in the bulk of the solution. Writing $\mu = \mathrm{RT} \log a + k$, where a = 'activity' of the solute, and k is a quantity dependent only on the temperature and nature of the solute and solvent, $d\mu = \mathrm{R\,T}\, d \log a$, and so Gibbs' equation can be written in the form

$$q = -\frac{1}{\mathrm{RT}} \frac{d\gamma}{d \log a}.$$

If for very dilute solutions (or for so-called 'ideal' solutions) we put $a = c$, we can write

$$q = -\frac{1}{\mathrm{RT}} \frac{d\gamma}{d \log c} = -\frac{c}{\mathrm{RT}} \frac{d\gamma}{dc}.$$

In this way Milner has calculated from Whatmough's data for aqueous solutions of acetic acid that the 'saturation' value of q is 3.3×10^{-10} mols. per sq. cm., from which it follows that the area per molecule in the surface is 50×10^{-16} sq. cm. In a similar manner, Langmuir has calculated from B. de Szyszkowski's data for aqueous solutions of propionic, butyric, valeric, and caproic acids that the surface area per molecule adsorbed in the saturated layer is equal to 31×10^{-16} sq. cm., whilst Harkins has arrived from his own measurements for butyric acid at the value 36×10^{-16} sq. cm.

In 1911 Dr. J. T. Barker and myself made a direct determination of q for a solution of nonylic acid in water. For a practically saturated surface layer it was found that q was about 1.0×10^{-7} grm. per sq. cm., or 3.1×10^{14} molecules per sq. cm. From this result it follows that the surface area per molecule is 26×10^{-16} sq. cm.

If we consider these various values, it will be at once evident that they are not very different from the values found by Langmuir and by Adam for the oriented unimolecular layers of practically insoluble fatty acids resting on the surface of water. That in the present case some of the values are larger might easily be explained on the ground that these adsorption layers are partially, or completely, in the state of 'surface vapours.' For Adam and Marcelin have recently made the important discovery that the unimolecular surface films investigated by them may pass rapidly on increase of temperature from the state of 'solid' or 'liquid' surface films to the state of 'vaporised' surface films, in which the juxtaposed molecules become detached from each other and move about with a Brownian or quasi-molecular motion,

probably communicated to them by the thermal agitation of the water molecules to which they are attached.

It is, indeed, highly probable that the molecules which are concentrated in the surface from the state of solution in the liquid phase are not in quite the same situation as the molecules of practically insoluble substances which are placed *on* the surface. In the former case the molecules are still 'dissolved,' so that they will be more subject to thermal agitation and less able to form a juxtaposed unimolecular layer. They may also be 'hydrated.' The difference between the two cases is rendered very evident from the fact that in the production of surface layers from dissolved molecules of the fatty acids (and other 'surface active' substances) there is a very marked fall of surface tension, whilst the uncompressed unimolecular surface films placed on the surface from outside do not affect the surface tension of the water. Thus the molecules of the surface-active substance in the former case are in much closer relation to the solvent molecules, and are in kinetic equilibrium with the molecules of both solvent and solute in the bulk of the liquid. Nevertheless, the agreement as regards order of magnitude in the values of the surface area per molecule in the two types of case is certainly very suggestive and significant. Moreover, the experiments of Mr. Iredale show that molecules which are adsorbed on the surface from the vapour phase lower the surface tension, and are therefore from this point of view comparable with molecules concentrated in the surface from the bulk of the liquid phase.

The question as to whether the simplified form of Gibbs' equation yields a sufficiently accurate value for the excess surface concentration can scarcely be decided without more experimental data. In the experiments made by Dr. Barker and myself, the values calculated from the surface tension-concentration curve were 1.3×10^{-7} and 0.6×10^{-7} grm. per sq. cm., according as the value of the van't Hoff factor i for the very dilute solutions of nonylic acid was taken as 1 or 2 respectively; whilst the corresponding directly determined value was about 1.0×10^{-7} grm. per sq. cm. This discrepancy was probably well within the experimental error of our measurements.

Let me now direct your attention to another very interesting phenomenon relating to the surfaces of liquids and solutions—namely, the existence of an electrical potential gradient or potential difference in the surface layer. These interfacial potential differences are of great importance, and play a fundamental rôle in determining the stability or instability of many colloidal states of matter. The liquid-gas interface offers the simplest case of such interfaces, and so the investigation of the potential differences which may exist at this interface is a matter of fundamental interest In 1896 F. B. Kenrick developed, on the basis of earlier experiments of Bichat and Blondlot, an electrometric condenser method for the comparative determination of the gas-liquid P.D.'s. The results which he obtained show that substances (such as the aliphatic alcohols and acids) which concentrate at the surface produce a very great change in the surface P.D., whilst highly dissociated univalent inorganic salts, such as KCl, do not. The results obtained by Kenrick have been much extended by an investigation carried out

with the same type of apparatus by Professor Thorbergur Thorwaldson in my laboratory. The general result of these experiments can be described in the following terms:—

Consider the system:

Aqueous Solution of KCl (conc. $=c$) A	Air	Aqueous Solution of KCl (conc. $=c$) B

The positive potential of A will be equal to that of B. If we now add to the solution B a small quantity of a substance S (generally a non-electrolyte or weak electrolyte) which has a strong tendency to concentrate at the air-B interface, it is found that the positive potential of A rises markedly above that of B, the value of the quantity, positive potential of A minus that of B, varying with the concentration of S in the way that is characteristic of adsorption phenomena. What is the interpretation of this phenomenon? If we were to assume that there was practically no P.D. at the interface A-air, it would follow that the effect of S is to make the positive potential of the bulk of B markedly below that of the air. The same result would follow if we were to assume that at the interface A-air there exists a P.D. which makes the positive potential of the bulk of A markedly below that of the air outside. Both these assumptions would lead to the conclusion that in the surface layer of the solution at the A-air interface there must exist either no electrical double layer, or else one with its *positive* half oriented towards the air side. Now Quincke has shown that a bubble of air in water placed in an electrical potential gradient travels towards the anode—*i.e.* the bubble behaves as if it were negatively charged. From this it would follow that the P.D. at the air-water interface is such that the *negative* half lies towards the air side. As an electrolyte such as KCl is negatively adsorbed at an air-liquid surface, it is probable that a P.D. of the character indicated by Quincke's experiment exists at the A-air interface. If we accept this conclusion, it follows that the effect of S is markedly to *reduce* this P.D. (or to reverse it). Now the P.D. at the air-water interface is probably due to the existence of a double layer containing hydroxyl ions on the outside and hydrogen ions on the inside, or to oriented water molecules regarded as electrical bi-poles. If S is a non-electrolyte (or a substance which possesses little self-ionisation), we can understand why its concentration at the surface could result in the reduction of this P.D.

The experiments of Thorwaldson show that a substance such as the hydrochloride of methyl violet has a powerful effect on the P.D. at the air-water interface. It is probable that in this case the complex basic dye cation is drawn into, or 'adsorbed' in, the outside layer next to the air, the result of this being a reduction or possibly reversal of the original potential difference.

Kenrick found that if gases such as hydrogen and coal gas be substituted for air, there is no effect on the surface P.D.

Within the last few years H. A. McTaggart has made a number of experiments on the electric cataphoresis of gas bubbles in aqueous solutions and other liquids. He finds that aliphatic acids and alcohols in

aqueous solution reduce the surface P.D. and that this effect runs parallel with their influence on the surface tension of water. He also finds that acids reduce the P.D. These results may be regarded as a corroboration of those obtained by Kenrick. McTaggart has found that the nitrates of tri- and tetravalent cations have a powerful effect in not only reducing but reversing the P.D. (*i.e.* the bubble becomes positively charged). His experiments also show that polyvalent negative ions, such as the ferrocyanide ion, act in the opposite direction to the polyvalent cations—*i.e.* they increase the negative charge on the bubble or diminish a previously existing positive one. These results are of great interest, inasmuch as they show the powerful effects produced by polyvalent ions on the P.D. existing in the surface layer of an aqueous solution. As we shall see presently, very similar results have been obtained at liquid-liquid and solid-liquid interfaces. But it is of great importance to know what happens at the air-liquid interface, since we can largely discount the chemical and physical influence of the gas phase.

Although the electrometric method employed by Kenrick and Thorwaldson only gives comparative results (since two interfaces must always be simultaneously used), whilst the cataphoresis method gives results for a single interface, it is necessary to observe that the electrometric method measures the *total* fall of potential from the bulk of one phase to the bulk of another. The cataphoresis method measures what Freundlich has called the 'electrokinetic' P.D.—that is to say, the potential drop between the limiting surface of the 'fixed' part of the double layer and the rest of the liquid. The two values need not necessarily coincide.

When liquids are sprayed or splashed, or when gases are bubbled through liquids, it is known that the gas often acquires an electrical charge, whilst the liquid acquires an opposite one (so-called 'waterfall' electrification). Since the pioneer work of Elster, Lenard, J. J. Thomson, Kelvin, Maclean, and Galt, very many investigators have dealt with this subject (Eve, Christiansen, Bloch, de Broglie, Zwaardemakers, Coehn, &c.). Originally, Lenard thought that the effect was due to a 'contact electrical' action between the gas and the liquid, whilst J. J. Thomson was inclined to ascribe it to a sort of partial chemical action between them. It is known that there are produced in the air or gas relatively slow-moving carriers, both positive and negative. Lenard has quite recently changed his views, and ascribes the origin of these carriers to the tearing off of very small portions of the outside layer of the electrical double layer existing in the surface of the liquid. It may be mentioned that Kenrick, Thorwaldson, and McTaggart came to the conclusion that the surface P.D.'s measured by them were not connected, or at all events not connected in any simple manner, with the phenomena of waterfall electrification.

We may say, therefore, that if there be a relation between these two types of phenomena, it is a complicated and still largely obscure one.

The subjects which I have been discussing have an interesting bearing on the formation and stability of foams and froths. If air be violently churned up with water, only comparatively large bubbles are produced,

and these quickly rise to the surface and burst. If now a very small quantity of a substance which concentrates at the air-water interface be added, an almost milk-white 'air emulsion' of small bubbles is produced, which rise to the surface and produce a relatively durable froth. These phenomena were discussed by the late Lord Rayleigh in a very interesting Royal Institution lecture on 'Foam.' It is clear that the diminution in interfacial tension facilitates the subdivision or dispersal of the air. The existence of the surface layer will also confer a certain amount of stability on the resultant froth, since it will give rise to forces which resist the thinning of a bubble wall. Any sudden increase in the surface will produce a momentary diminution in the concentration or 'thickness' of the surface layer, and hence a rise in surface tension, which will persist until the normal thickness or concentration is readjusted by diffusion of molecules from the inside volume—a process which in very dilute solution will occupy a perceptible time. That this explanation (due to the late Lord Rayleigh) is the correct one can be seen from the fact that very often stronger solutions of the same surface-active substance scarcely foam at all. In this case the readjustment of the equilibrium thickness or concentration of the surface layer occurs with such rapidity (owing to the greater concentration of the molecules in the inside volume) that practically no rise in surface tension, and hence no counteracting force, comes into play. These effects will be the more pronounced—other things being equal—the greater the mass and hence the smaller the motion of the solute units, as in the case of large molecules or colloidal micelles. It is probable, however, that the explanation of the stability of very durable forms, as, for example, those produced by the sea at the sea coast, by beer and stout, by aqueous solutions of soap or saponin, &c., is often more complex, and that we must seek it in the formation of very viscous or semi-rigid or gel-like membranes at the interface. Moreover, small solid particles may contribute to the stabilisation of a froth, as in the case of the 'mineralised froths' of the ore flotation process; and the preferential aggregation of small particles in the interface between two phases has been demonstrated in the experiments of W. Reinders, F. B. Hofmann, and many others.

Let us now inquire how far the phenomena which we have seen to be characteristic of a gas-liquid interface occur also at the interface between two immiscible or partially miscible liquids. Many years ago it was shown by Gad and by Quincke that a fatty oil (such as olive oil) is very readily dispersed in the form of an emulsion by a dilute solution of caustic soda. Some experiments which I once made showed that a neutral hydrocarbon oil could be similarly emulsified in a dilute aqueous solution of alkali if one of the higher fatty acids was dissolved in it, whilst the lower fatty acids do not produce a similar action. It was shown that the action runs parallel to the lowering of interfacial tension and must be ascribed to the formation of a soap, which lowers the interfacial tension and concentrates at the interface. These phenomena have have been further investigated by S. A. Shorter and S. Ellingsworth, by H. Hartridge and R. A. Peters, and by others.

If a substance which is dissolved in one liquid A, and which is

practically insoluble in another liquid B, is found to have, in very dilute solutions, a strong effect in lowering the tension at the interface A-B, the following interesting questions arise:—

(1) What is the amount of the surface concentration or adsorption per sq. cm. of interface?

(2) Can it be calculated by means of the simplified Gibbs equation?

(3) How does the surface adsorption vary with the concentration?

(4) Does the 'saturation' value correspond to the formation of a unimolecular layer?

Some of these questions were experimentally investigated in my laboratory by W. C. McC. Lewis. For the liquid A water was chosen, and for B a neutral hydrocarbon oil. Working with sodium glycocholate as the surface-active subtance, it was found that the experimentally measured surface adsorption q was much greater than that calculated by means of the equation

$$q = -\frac{c}{RT}\frac{d\gamma}{dc}.$$

For example, a 0.2 per cent. aqueous solution at 16° C. gave a directly measured value of $q=5\times10^{-6}$ grm. per sq. cm., whilst the calculated value was 5×10^{-8} grm. per sq. cm., practically a hundred times smaller. A similar type of discrepancy was found in the cases of Congo Red and methyl orange. If we calculate from the experimentally found surface adsorption of sodium glycocholate the value of the surface area per molecule, we obtain about 0.9×10^{-16} sq. cm. A similar calculation in the case of Congo Red gives a correspondingly low figure. Now if we compare these values with those previously obtained for the air-liquid surface, it is clear that in the present case we are not dealing with simple unimolecular layers, but with adsorption layers or films many molecules thick. On the other hand, if we calculate from Lewis's results the surface area per molecule as deduced from the surface tension measurements by the simplified Gibbs formula, we arrive at values of the order of 90×10^{-16} (sodium glycocholate) and 100×10^{-16} (Congo Red). These are values which are consistent with the gradual building up of a unimolecular layer (of possibly heavily hydrated molecules or micelles). It is possible, therefore, that the Gibbs equation gives the surface concentration of the primary unimolecular 'two-dimensional' surface phase, and that any building up of further concentrations beyond this layer does not affect the surface tension. It is true that in the case of substances such as sodium glycocholate, and especially Congo Red, in aqueous solution, there is a considerable amount of uncertainty as to the nature and molecular weight of these substances as they exist, not only in the bulk of the solution, but especially in the surface phase. In a later investigation Lewis determined the surface adsorption of aniline at the interface mercury-aqueous alcoholic solution, and found in this case a very fair agreement between the observed and calculated results. This case is more favourable, since we can be in little doubt concerning the molecular weight of the solute units. The mean observed value for the surface adsorption was

2.7×10^{-8} grm. per sq. cm. Hence the number of molecules per sq. cm. of interface

$$=\frac{2{\cdot}7}{93}\times 10^{-8}\times 6{\cdot}06\times 10^{23}=0{\cdot}17\times 10^{15},$$

and the surface area per molecule $=58 \times 10^{-16}$ sq. cm. Langmuir's calculation from Worley's measurements of the surface tensions of aqueous solutions of aniline gives at the air-water surface the value 34×10^{-16} sq. cm. for the area per molecule of aniline. We may conclude, therefore, that Lewis' measurements in this case point to the building up of a primary unimolecular layer, unaccompanied by any further concentration or 'condensation' of molecules or colloidal micelles.

The relation between surface adsorption and fall of interfacial tension at a mercury-water interface was further investigated by W. A. Patrick, who concluded that, although there was a correspondence between the two phenomena, the surface adsorption could not be calculated from the simplified Gibbs equation. If we were to accept this conclusion as correct, we might find an explanation either in the suggestion made above, or in the possible invalidity of conclusions drawn from the use of the simplified Gibbs equation; either because the simplifications introduced are not justified, or because the existence of electrical or other factors requires an extension or elaboration of the original equation. This matter has been discussed by Lewis, by A. W. Porter, and by various investigators of electro-capillary phenomena.

From very accurate measurements of the interfacial tensions of the aqueous solution-mercury interface, W. D. Harkins has calculated (by means of the simple Gibbs equation) that when the interface is saturated as regards butyric acid molecules coming from the aqueous solution, the surface area per molecule is 36×10^{-16} sq. cm.

Here, again, we see that a calculation by means of the Gibbs equation seems to point to the formation of a primary unimolecular layer. Experiments similar to those of Lewis have been very recently made by E. L. Griffin, who has measured directly the adsorption of soaps from aqueous solutions at a mineral oil-water interface. The results obtained are as follows:—

Substance	Average Surface per Molecule adsorbed
Sodium Oleate	48×10^{-16} sq. cm.
Potassium Stearate	27×10^{-16} sq. cm.
Potassium Palmitate	30×10^{-16} sq. cm.

These figures are very interesting, for they would appear to indicate the formation of unimolecular surface layers. It may be mentioned here that T. R. Brigges has investigated the adsorption of sodium oleate at a benzene-water interface, and finds that the amount of soap adsorbed at the interface increases rapidly at first with small increases in the concentration of the solution, and then remains very nearly constant while the concentration of the solution undergoes great increase. This is just what one would expect from the building up of a saturated surface or surface layer (whether unimolecular or otherwise).

We have seen that in the case of the air-water surface there exists an electrical separation or potential difference in the surface layer, and that certain substances can produce pronounced variations, or even reversals in sign, of this electrical double layer. It becomes a matter, therefore, of great interest to inquire whether similar phenomena occur at the interface between two immiscible liquids, and, if so, to ascertain whether such electrical charges or double layers bear any relation to the 'stability' of pure emulsions, or fine dispersions of one liquid in another. It is well known that those disperse or finely heterogeneous states of matter known as colloidal solutions depend in part for their stability on the existence of such electrical potential differences. We might expect, therefore, that an investigation of these emulsion systems would throw some light on the general theory of what are called 'suspensoid' or 'lyophobic' colloidal states. Investigations with these objects in view were carried out some years ago in my laboratory by R. Ellis and F. Powis. The method employed was to measure directly by means of a microscope the motion of minute globules (suspended in water) under the influence of a known electric field. This procedure may be regarded as an extension and development of the work of Quincke. From the measured velocity and potential gradient the interfacial P.D. and the electrical charge can be calculated from the theories of Helmholtz, Lamb, and Stokes. The microscopic method has the advantage that the P.D. between the aqueous solution and the glass wall (cover-glass or object glass) can be simultaneously determined It is a remarkable fact that the P.D. between various types of hydrocarbon oils (purified from acid as far as possible) and water was found to be 0.045—0.053 volt, the oil being negative—that is to say, the oil droplet moving towards the anode. If we compare this with the value recently calculated by McTaggart for the P.D. between an air-bubble and water (deduced from a precisely similar type of measurement), namely 0.055 volt, we can draw the conclusion that the potential difference is due to an electric double layer residing in *the surface layer of the water*. The oil droplet moves, therefore, with an attached negative layer or surface sheet, probably determined by hydroxyl ions, this being balanced by a positive layer whose charge is determined by hydrogen ions. If hydrochloric acid be added to the water the interface P.D. rapidly falls, and appears asymptotically to approach zero. If, on the other hand, caustic potash be added, the P.D. at first rises, reaches a maximum at a concentration of about one thousandth molar, and then falls with increasing concentration, but nothing like so sharply as in the case of the acid. Similar results hold good for the glass-water interface. From the results recently obtained by H. R. Kruyt (by means of the stream method) it is probable that at very low concentrations of acid there also occurs an initial increase in the interfacial P.D. The influence of salts is very remarkable. Thus at low concentrations potassium chloride and potassium ferrocyanide increase the P.D., whilst at higher concentrations they reduce it, just as in the case of the acid and the alkali. The initial increase caused by potassium ferrocyanide is markedly greater than that caused by potassium chloride. The effect of the valency of the salt cation is very pronounced. Thus barium chloride at very low

concentrations probably causes a very small rise of the P.D., but at quite low concentrations its effect is to reduce it, and this effect increases rapidly with rising concentration, and is much more marked than in the case of potassium chloride. The lowering effect of aluminium chloride at low concentrations on the P.D. is much more pronounced than in the case of barium chloride, and this effect becomes still greater with thorium chloride. Both aluminium chloride and thorium chloride at low concentrations *reverse the sign* of the P.D., the oil side of the double layer becoming positive. In these cases the positive charge of the oil droplet reaches a maximum with increasing concentration of the salt, and then appears to fall slowly towards zero. No second reversal of sign has ever been observed. So far as the solid-liquid interface is concerned, these results have been in general confirmed by the electroendosmotic experiments of G. v. Elissafoff (carried out in Freundlich's laboratory) and by the stream-potential measurements of Kruyt. It may also be remarked that Loeb has recently obtained similar results in the case of collodion particles, using the micro-cataphoresis method. Perhaps the most remarkable result which has emerged from these electrical investigations of oil suspensions is the relation between the stability of the emulsion and the potential difference of the interfacial double layer. The minute oil globules are in constant Brownian motion and must frequently collide. Why do the forces of cohesion not produce agglomeration or coalescence (coagulation or clearing of the emulsion)? We should expect that under determinate conditions a certain fraction of these collisions would give rise to coherence. Is there any other factor besides orientation of path and kinetic energy which affects the probability of coherence following an encounter? At distances great in comparison with their own dimensions the electric double layers will act practically as closed systems. But when two oil drops approach sufficiently near each other the conditions will be different, since we must expect a repulsive force when two similarly charged outer layers just begin to interpenetrate each other. Hence the answer to the question asked above is that the third factor is the potential difference or electric density of the interfacial double layer. Other things being equal, the probability P of an encounter leading to coherence will be a diminishing function of the electric intensity π of the similarly constituted double layers, *i.e.* $\frac{dP}{d\pi}$ will be negative. Hence of the total number of encounters in a given small period of time the number which lead to coherence should be a maximum at the point of zero potential difference (iso-electric point of Hardy).

Now the experiments of Powis brought out the very important fact that when the interfacial P.D. (whether positive or negative) is above a certain value, which was about 0.03 volt for his conditions, the rate of coagulation or coherence of the oil drops is relatively small, but rapidly increases when the P.D. falls inside the zone -0.03 to $+0.03$ volt. Under definite conditions there exist, therefore, what we may, speaking broadly, call a *critical potential* and a *critical potential zone*. When the P.D. is outside this zone the emulsion is comparatively very 'stable.' Very small concentrations of electrolytes, which, as we

have seen, increase the P.D., increase this stability. As soon as the concentration of any electrolyte is sufficient to bring the P.D. into the critical zone, the stability of the emulsion undergoes a sudden and very marked decrease, and relatively rapid coagulation occurs. Take, for example, the case of thorium chloride. On increasing the concentration we find that the interfacial P.D. traverses successively the following regions:—

(1) Above the critical value (and negative).
(2) Inside the critical zone (negative and positive).
(3) Above the critical value (and positive).
(4) Below the critical value (and positive).

In exact correspondence with this series we find that the emulsion goes through the following states:—

(1) Stable (oil particles 'negative').
(2) Unstable and flocculating (oil particles negative or positive).
(3) Stable (oil particles positive).
(4) Unstable and flocculating (oil particles positive).

Here we see a very striking analogue and explanation of the phenomena observed by Joly in studying the effect of aluminium salts on the sedimentation of clays, and of the numerous examples of the so-called 'irregular series' observed in the flocculation of suspensoid hydrosols by salts with polyvalent cations.

As Linder and Picton showed, when two suspensoid hydrosols, one negative and the other positive, are mixed, then, depending on the ratio, a stable hydrosol (either positive or negative) can be obtained. In continuation of this work, W. Biltz demonstrated the existence in such cases of a 'zone of coagulation,' *i.e.* a zone of concentration ratios leading to coagulation. A study of the mutual behaviour of a negative oil emulsion and the positively charged ferric oxide hydrosol provides a complete explanation of this curious phenomenon. When increasing amounts of the iron oxide hydrosol are added to the oil emulsion it is found that the interfacial P.D. falls to zero, and then reverses its sign, becoming increasingly positive—an action which is due to the adsorption of the positively charged micelles at the oil-water interface. When the P.D. is above a certain value (positive or negative) the system is stable. But within the critical zone a rapid and relatively complete mutual coagulation takes place.

These studies of oil emulsions (and of the glass-water interface), by means of the micro-cataphoresis method, have thrown a great deal of light on many previously ill-understood points in the theory of colloids. If, for example, the P.D. between the particles of a suspensoid hydrosol and the aqueous fluid are not above the critical potential, coagulation will occur. But very small concentrations of certain electrolytes can raise the P.D. and stabilise the hydrosol. This is the explanation of the well-known 'peptising' action. Higher concentrations of even the same electrolytes will reduce the P.D. below the critical potential, and produce flocculation. We see also that rapid coagulation will occur before the P.D. becomes zero. This was proved for arsenic sulphide hydrosol by Powis. Later experiments of Kruyt have confirmed these conclusions. It is obvious, therefore,

that coagulation of a lyophobic hydrosol will occur before the iso-electric point is reached, and that Hardy's famous rule requires revision.

The following table contains the concentrations (in millimols per litre) of certain electrolytes required to reduce the potential of a certain hydrocarbon oil emulsion from its 'natural' value (against pure water) of 0.046 volt to the critical value, 0.03 volt:—

—	Concentrations	Ratios of Concentrations
KCl . . .	51	2500
$BaCl_2$ . . .	1·9	95
$AlCl_3$ . . .	0·020	1
$ThCl_4$ . . .	0·070	0135

These results show the enormous influence of the valency of the cation in a series of salts with the same univalent anion, and explain in a striking manner the analogous effects in the coagulation of lyophobic hydrosols. The exact value of the critical potential and the range of the critical zone will depend, of course, on the experimental definition of 'rapid coagulation,' and on the concentration, nature, and degree of dispersion of the hydrosol. It is not to be supposed, therefore, that these critical values are constants except under very definite conditions. The fundamental fact is that under given conditions the rate of coagulation of the particles of an oil suspension or of a lyophobic hydrosol undergoes a relatively sudden and very great increase when the interfacial P.D. falls below a certain finite value (positive or negative).

There is not time or space at my disposal to enter into the much discussed question as to the inner mechanism of the action whereby ions (and electrically charged micelles) set up or vary the potential difference in the interfacial layer. According to Freundlich's original theory we must ascribe an independent effect to each ion, which will depend on the sign of its charge, its specific adsorbability, and electrovalency and the nature of the already existing double layer. A different theory was proposed by Freundlich in order to explain the results obtained in the electroendosmotic experiments of Elissafoff. According to this point of view, the 'solid' surface acts chemically (as an acid, base, ampholyte, or salt), whereby it may dissociate off an ion or ions, and itself become an ionised surface. Invading foreign ions may then alter this ionisation equilibrium; or they may simply combine with the ionised surface and form neutral insoluble spots (compare the views of Freundlich, Gyemant, and Kolthoff). J. N. Mukherjee has suggested that ions are attached to the surface by chemical forces, and has attempted to work out an electro-kinetic theory of ion adsorption. It is probable that surfaces very often do act ionically or chemically, and that specific actions of this sort must often be taken into account in dealing with the great variety of material presented in the study of surface actions. Nevertheless, in the case of the hydrocarbon oil droplets studied by Ellis and Powis, or the gas-liquid interface studied

by Kenrick, Thorwaldson, and McTaggart, any *specific* chemical activity or ionisation of the oil or gas would seem improbable. Any theory which attempts a general treatment of the problem must be prepared to deal with cases such as these.

Many measurements have been made of the potential differences between solids and liquids, or between pairs of immiscible (or partly miscible) liquids, using electrometric methods. Thus Haber and Klemensiewicz determined the potential difference at a glass-water solution interface, and found the glass to act like a hydrogen electrode. Their results have been recently confirmed by W. S. Hughes. It will be at once obvious that these results are not in agreement with those obtained by cataphoretic and electroendosmotic methods. A somewhat similar type of discordance has been observed in the electrometric measurements of the potential difference between solid paraffin and an aqueous solution made by G. Borelius, and of the P.D.'s between pairs of liquids made by R. Beutner, E. Baur, and others. Freundlich and Gyemant have drawn attention to the fact that in all such electrometric measurements, where in the process of the measurement an electric current must pass from one phase to the other, we measure the total or 'thermodynamic' potential difference between the phases in bulk, whereas in determinations by the methods of electroendosmose and cataphoresis, we measure only a portion of this total potential difference. These 'electro-kinetic' P.D.'s, although of fundamental importance in relation to the stability of suspensoid (lyophobic) systems, need not, and in general will not, coincide in value with the total (thermodynamic) potential differences. It will be recollected that I drew attention to a quite analogous difference in discussing the measurements of the potential differences at gas-water interfaces made by Kenrick and by McTaggart.

We may illustrate this point by considering the P.D. between two immiscible phases, L_1 and L_2, in equilibrium with each other, and each of which contains dissolved in it the electrolyte KA. If ε denote the positive potential of L_2 above L_1, and F the quantity of electricity associated with an ionic gram equivalent, then by a virtual variation of the equilibrium system it follows that

where the subscripts refer to the cation or anion and to the phases L_1 or L_2, and the μ's denote the chemical potentials per gram equivalent (partial equivalent free energies) of the ionic constituents in the bulk of the two phases. Whatever may be the 'electro-adsorption' or ion adsorption of K and A at the interface L_1-L_2, it is clear that ε depends only on the 'bulk' values of the respective chemical potentials, which likewise determine the surface concentrations. If the phases L_1 and L_2 be not in equilibrium, then velocity or diffusional terms will enter into the equations, and the potential difference will be partly or wholly a 'diffusional potential.' These relationships were clearly established many years ago by R. Luther.

In discussing the 'stabilities' of hydrocarbon oil emulsions, it must not be forgotten that I was dealing with very dilute *suspensions* of oil

in water, produced by mechanical agitation without the addition of any 'emulsifier.' I pointed out that in the emulsification of oils in water by means of soap, the soap lowers the interfacial tension and concentrates at the interface. When we wish to produce oil emulsions in the ordinary sense of the term we must use some such emulsifying agent, and for this purpose many substances are employed, such as soap, gum acacia, gelatine, casein, starch, &c., &c. All these substances concentrate or condense on the surfaces of the oil globules. If we may regard these surface films as very mobile from the molecular-kinetic point of view, it is clear that they will confer an increased degree of stability on the emulsion. For any sudden decrease of interface (caused, for example, by coalescence or partial coalescence of two adjacent globules) will produce a momentary increase in the surface concentration or thickness of the adsorption layer, and so a decrease in the interfacial tension, if the surface layer is not saturated. It may require a perceptible time for the molecular-kinetic motion (especially in the case of large molecules or hydrated micelles) to readjust the equilibrium between the surface layer and the bulk.

It is probable, however, that the stability of the emulsion is in many cases due to the fact that the surface films possess a very viscous, quasi-rigid, or gel-like character, so that a more mechanical explanation is necessary. As S. U. Pickering showed, oils may be emulsified in water by the gels of certain basic salts; and A. U. M. Schlaepfer has shown that emulsions of water in kerosene oil may be obtained by means of finely divided 'carbon.' Nevertheless, even in cases where an emulsifier is used, we may hope to succeed in obtaining a more precise physical analysis of the system. It is interesting in this connection to note that Mr. W. Pohl has recently found in my laboratory that when a neutral hydrocarbon oil is emulsified in water by means of sodium oleate, the electrical potential difference at the oil-water interface is almost doubled, and that the effects of alkalies and salts on this potential difference are very similar to those found in the case where no emulsifier is employed.

I cannot conclude this account of certain aspects of surface actions and properties without making a passing, though all too brief, reference to the beautiful investigations of Sir George Beilby on the amorphous layer. He has shown that when the surface of crystalline matter is subjected to shearing stress there is produced a surface layer of a vitreous or amorphous character—a 'flowed' surface—in which the particular ordered arrangement of the molecules or atoms which is characteristic of the crystalline matter largely disappears. Working at University College, London, Dr. Travers and Mr. R. C. Ray have recently obtained a very interesting confirmation of the Beilby Effect. The heats of solution (in kilogram calories per gram mol) of vitreous silica and silver sand (silica as crystalline quartz) in aqueous hydrofluoric acid were found to be 37.24 and 30.29 respectively. After grinding for fifteen hours the corresponding values were 36.95 and 32.46 respectively. If we assume that the internal energy of the amorphous phase produced by grinding is the same as that of the vitreous silica (silica glass), we can calculate from these results that about 31 per

cent. of the crystalline silica has been converted by grinding into 'amorphous' silica. The densities of silica glass and silver sand were found to be 2.208 and 2.638 respectively. After fifteen hours' grinding the density of the latter was lowered to 2·528. On the same assumption as before, it follows that about 26 per cent. of the quartz has been converted into the vitreous condition. The difference between the figures 31 and 26 is doubtless due to the approximate character of the assumption underlying the calculations and to experimental errors. There seems little doubt, however, about the soundness of the main conclusion—namely, that the mechanical action of shearing stress on crystalline matter is to produce a random molecular or atomic distribution in the surface layers.

This discussion, necessarily brief and limited, of certain aspects of the properties of surfaces—molecular orientation, surface concentration or adsorption, electrical or ionic polarisation—has dealt very largely with states of thermodynamic equilibrium. The chief interest of such studies has always appeared to me to lie in their possible ultimate bearing on the phenomena of life. We must remember, however, that the activities, and indeed the very existence, of a living organism depend on its continuous utilisation of an environment that is not in thermodynamic equilibrium. A living organism is a consumer and transformer of external free energy, and environmental equilibrium means non-activity, and eventual death.

It is probable, therefore, that along and across 'living surfaces' there is a continual flux of activity. Does the very existence of these surfaces depend on some special sort of activity? Questions such as these must make us cautious as regards any premature generalisation from simple physico-chemical results. But there is encouragement if we may assume that the physico-chemical manifestations of life are functions of the same powers and potentialities of electrons, atoms, ions, and molecules that we find in what we call inanimate environments. Life would then be simply a new functional relationship of very old parameters, at all events in so far as its various physico-chemical 'mechanisms' are concerned.

In the totality of its activities and relationships, however, a living organism is an individual, and to arrive gradually at an understanding of this 'individualisation' it will be necessary to study very carefully the laws pertaining to the intimate and particlar modes of action of simpler individuals. The actions of an individual are conceived by science as determined by its internal state and by its relation to its environment. As we pass from certain peculiar atomic states, where the actions appear to have no relation to environment, to molecules, colloidal micelles, and living cells, the effects of the environment in determining activity seem to become more and more pronounced.

The internal state of a living cell or organism may arrive from time to time at 'critical' points and 'critical' transformations. Whatever may be the relation of such possible critical states to the previous cell-environment reactions, the resulting events will be immediately determined by the special internal nature and activity of the cell itself. Is this 'special internal nature and activity' simply a special type of

organisation or arrangement of the positions, shapes, sizes, orientations and motions of electrons, atoms, ions, and molecules? To this oft-put question the answer of physico-chemical science is still in the affirmative. More complex individuals are not cloaked in any mysterious 'law of complexity.'

Probably future progress will depend more on the investigation of the special nature, situation, and action of individuals than on the statistical thermodynamic treatment of the average behaviour of the 'crowd.'

EVOLUTIONAL PALÆONTOLOGY IN RELATION TO THE LOWER PALÆOZOIC ROCKS.

ADDRESS BY

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PRESIDENT OF THE SECTION.

It is just twenty-seven years since the British Association last met in Liverpool, and in casting my mind back over the intervening years and thinking how our Science stands to-day with regard to its position then, it has appeared to me that one at any rate of the most important lines along which progress has been achieved is due to the growth of what may be termed the genetic principle. This would seem to be equally true both as regards Petrology and Palæontology, for it is becoming increasingly evident that conceptions and classifications, whether they be of rocks or of fossils, if they are to be natural, must be based fundamentally upon origin and descent. Therefore, situated as we are here in Liverpool, almost within sight of the Welsh Hills on the one hand and the Lake District Fells on the other, both classic areas so far as the Lower Palæozoic rocks are concerned, it may perhaps be appropriate to see how far this principle may be applied to the elucidation of problems connected with these Lower Palæozoic rocks, to note what has been achieved in this respect, and how much yet remains to be done. The subject, therefore, of my address to you to-day is 'Evolutional Palæontology in Relation to the Lower Palæozoic Rocks.'

Problems of the Older Rocks.

As I interpret the facts, the chief problems still awaiting solution are both fundamentally stratigraphical; on the one hand there are problems relating to classification, that is, of subdivisions of the formations on a basis that shall be of wide application, and render possible correlation of beds in areas far removed from one another; on the other, there is the actual structural relationship existing between these beds as seen in the field, which, when rightly interpreted, makes evident the nature and extent of those deformational strains that have from time to time so profoundly affected the rocks of the Earth's crust. With regard to classification, the problems are of different degrees of magnitude; there are, for example, those larger difficulties relating to the satisfactory determination of the upper and lower limits of the formations; there are also those connected with the correlation of all those smaller local subdivisions of formations with which Stratigraphical Geology is becoming increasingly overburdened without any prospect of compensation unless a fresh principle be introduced.

Moreover, the interpretations of structural details must to a large extent depend upon the satisfactory elucidation of these problems of classification, so that the solutions of the two really go together. It is my firm conviction that the most satisfactory solution of the first, and therefore also of the second, of these problems will be found in the application of the principles of evolutional Palæontology.

Variation in Shallow-water Fauna.

As regards the more fundamental of the two problems, a general principle seems to be involved, demanding the recognition of the relative values of faunal changes in shallower and deeper waters respectively. The faunas of the shallow seas must of necessity be subject to far greater degrees of physical change than those of the deeper waters, and, thanks to the excellent work done at the Danish Biological Station [1] in carrying out investigations on the bottom faunas of different Danish waters, we now know a good deal as to the extent to which the distribution of modern faunas is governed by physical conditions. Some of the more important conclusions reached by the Danish investigators may be summarised as follows:—

1. That certain *characteristic* animal communities undoubtedly exist under certain physical conditions, and when these conditions remain constant even over wide areas the same community will be found, but each community is bounded by those physical conditions.

2. That change in physical conditions brings about a change in the *characteristic* animal community, though certain organisms may be found in more than one community.

3. The physical changes to be correlated with the change in community are those of temperature, salinity, and clearness of the water; depth as depth seems to be less important than *the factors which go with depth*, such as temperature, amount of light, character of the sea bottom, and quietness of the water. Thus along a section in the N. Kattegat at depths varying only between 7 and 50 metres, five different animal communities have been recognised:—

Community	Depth	Character of Bottom	Temp.
1. Echinocardium Community .	7 metres	Fine sand	—
2. Echinocardium - Turritella Community	12-19 metres	Dark sand with fine detritus	—
3. Brissopsis - Turritella - Echinocardium Community (transition)	24.5 metres	Fine sand with fine particles	14° C.
4. Brissopsis - Turritella Community	35 metres	Grey Kattegat Clay	13.4° C.
5. Brissopsis-Nucula Community	50-52 metres	Light Kattegat Clay	8°—6° C.

[1] 1913, Petersen, C. G. J. Report of the Danish Biological Station.

That depth is not the determining factor is clearly indicated by another section taken in the Samsø Belt:—

Community	Depth	Character of Bottom	Temp.
1. Macoma Community (No Echinoderms)	8 metres	Pure sand	18.5° C.
2. Calcarea Community . .	18 metres	Light mixed clay and sand	10.1° C.
3. Rich Modiola Echinoderm Community	18 metres	Coarse gravel with sand, clay, pebbles	10.3° C.

This Macoma community is very well known, as it occurs in all the more sheltered waters of the Danish Fjords, and can be directly observed and examined at low-water. It is seen to present many facies, and to vary greatly according to whether the bottom is sandy, stony, muddy, or soft, and according to whether it is exposed to the action of currents and to varying conditions of temperature and salinity. The fauna in bulk, apart from those characteristic species which belong to the community as a whole, varies considerably in different localities, and, as the author of the Report expresses it, 'the real matter for wonder is that there are some species common to all these localities and different conditions.'

The difference between the *characteristic* animals of the communities living in waters of different depth is so great that none of the animals are common to both. This does not mean that no species are common to shallower and deeper waters, but that no *characteristic* species as such.

Now the interest for the geologist in all this lies in the fact, as Petersen has pointed out, that these 'characteristic animals' are closely akin to the 'characteristic fossils' of the geologist, and we may ask ourselves whether the variations which can be seen to exist in the contemporaneous shallow-water fossil assemblages of past ages may not be recognised as brought about by the same factors as those that can be seen operating to-day.

Our ancient Lower Palæozoic faunas were composed in the main of trilobites, brachiopods, and corals, both solitary and reef-building. The distribution of coral reefs at the present day is governed by three cardinal factors[2]:—

(1) Uniformly warm waters, the temperature of which does not fall below 22° C. on an average throughout the year;

(2) A depth not exceeding 14 fathoms;

(3) Clear waters, *i.e.* those free from mud in suspension;

and there is every reason to believe that formations containing the remains of coral reefs were laid down under very similar conditions to these; hence reef-building corals might be expected to have flourished

[2] 1923, Potts, F. A. 'The Distribution of Coral Reefs.' *School Science Review*, Feb. 1923.

best in clear, warm seas of moderate depth, and such trilobites and brachiopods as occur abundantly associated with them might be presumed to have flourished also under these conditions; other brachiopods and trilobites would appear to have attained their greatest development on sandy shores, whilst others, again, seem to have lived in greatest numbers in muddy waters, no great change in depth being necessitated. Unfavourable conditions seem to be indicated by the dwarfing of a fauna as a whole, the extreme of such conditions being attained when salinity of the waters resulting from desiccation reached such a pitch that abnormally distorted forms predominate. Moreover, in the case of faunas inhabiting the actual coastal region, it is obvious that even slight changes in the relative levels of sea and land will be very effectively felt, since these may be sufficient to bring about permanent submergence or emergence, and thus induce a total change of environment; also since shore lines tend more particularly to be the lines along which migrations take place between the faunas of one area and another a further factor contributing to heterogeneity may thereby be introduced.

It is therefore obvious that there are many factors tending to give different aspects to shallow-water faunas in different places, hence a certain amount of *Lateral Variation* or lack of uniformity is only to be expected amongst those of the same age when seen in different localities. There may also be considerable *Vertical Variation* in the type of shallow-water fauna of successive ages, either in response to changes in the physical conditions of one and the same area, or as the result of migration; and if the surroundings are variable it may happen that two faunas separated in time may bear a greater degree of resemblance to each other than two successive faunas, the similarity being induced by a return to similar conditions. *Vertical Variation* has long been recognised and understood in principle by geologists, but I do not think the same can be said of *Lateral Variation*.

Since then a greater or lesser degree of variation is to be expected even in ancient shallow-water faunas that are contemporaneous, it is in their case too the resemblances that should be considered remarkable rather than the differences, since close resemblance would seem to indicate one of two things, either a wonderful degree of uniformity of conditions over a wide area, or else the occurrence of a large proportion of those fossils that I have elsewhere [3] called 'successful types,' these possessing amongst other characteristics the property of being, in some cases at any rate, less susceptible to differences of physical condition. Thus in comparing the contemporaneous shallow-water faunas of the past in different areas it would seem that all we are entitled to expect is a *general* resemblance rather than a *particular*, and this at best will probably show itself in generic rather than specific agreement, a fact well illustrated by the trilobite faunas of the Keisley and Chair of Kildare Limestones of Ashgillian age; the general aspect of these faunas is really far closer than any fossil list would indicate, since the species are often different, though the faunas agree in the

[3] 1922, Elles, G. L. 'The Graptolite Faunas of the British Isles.' *Proc. Geol. Ass.*, 1922, vol. xxxiii.

occurrence of numerous Cheirurids, Lichads, and Remopleurids. In many cases, therefore, where there is considerable lateral variation in faunas, and also where the fragmentary condition of the specimens renders specific determination impossible, or where the assemblage is insufficient to determine the horizon, the recognition of the evolutional stage reached by an organism may prove to be of greater significance than specific determination, in that it is essentially independent of any nomenclature, however much such a nomenclature may in the past have been forced upon it.

Moreover, since all shallow-water faunas will be liable to be affected to a greater or lesser extent by the different factors enumerated above, difficulties in correlation are bound to occur; these may be obviated in two ways, either by studying the faunas from the evolutional standpoint, and noting the stage reached, or by determining where possible the relation of each separate fauna to its deeper-water equivalent; for it is obvious that the faunas of the deeper-water areas where conditions are more uniform should furnish the standard for purposes of classification. Since the physical conditions in such areas are far more constant, and the sediments of more uniform type, any change in the character of the fauna that does take place is almost bound to be of real significance, and probably in many cases indicates the attainment of an important stage in the evolution of the group or groups of organisms concerned. Every modern classification of strata should surely take these data into account. It is not that I undervalue the importance of local changes—they have their own significance—but just because they are bound to be more or less entirely local they are useless for purposes of international correlation.

Principles of the Modern Classification of Strata.

It can hardly be doubted at the present day that the most efficient classification of strata is that based upon the palæontological principle of the coming in of new forms, but if the classification is to be of wide application and to be depended upon, this coming in of new forms must not be directly connected with changes in the character of the sedimentation. Physical causes which induce changes in the nature of the sediments are no doubt important, and probably give great impetus to evolutionary development, but to be depended upon they must be reflected in those faunas of the deeper parts of the epicontinental seas where sedimentation continues apparently unaltered; in other words, where the change in the fauna shows primarily as an advance in the evolutional stage. The factors that have to be considered render the international classification of our great formations a matter of considerable difficulty. This is well illustrated by the differences of opinion that exist as to where the upper limit of the Silurian should be placed, and in spite of all that has been urged by Stamp,[4] I am not yet convinced that his claim that the boundary should be shifted to the base

[4] Stamp, L. D. { 1920. *Geol. Mag.*, vol. lvii, p. 164.
1922. *Bull. Soc. Belge de Géologie*, vol. xxxi, p. 87.
1923. *Geol. Mag.*, vol. lx, p. 92 and p. 276. }

of the Downtonian rests upon a satisfactory basis. Towards the close of the Silurian, as is perfectly well known, far-reaching changes in physical conditions took place, necessarily involving changes in the character of the shallow-water fauna whenever and wherever these occurred, and the coming in of fishes appears to be directly connected with them. That these changes took place simultaneously over wide areas is in the highest degree improbable, and having had some experience of the behaviour of these rocks in the field I have felt that the evidence at times so strongly suggested that the Downtonian was essentially a *facies* formation that the possibility of its horizon being eventually found to be almost as inconstant as that of the Millstone Grit was far from improbable. That there may appear to be a similar change of conditions in parts of Britain and France at about the same time is not really the point; it is not the succession of shallow-water marine faunas that is important from the point of view of classification, but how far these are really of the same age in different places, and how much change is reflected in the fauna of the more stable deeper-water beds. The author may be perfectly right in his contention, only up to the present as I see the problem he has not proved his case.

So, too, at the lower limit of the same Silurian formation; at present the top of the Ashgillian needs clearer demarcation, and I have endeavoured to show elsewhere that on palæontological grounds the most satisfactory place at which to draw the line is at the horizon where Monograptus makes its first appearance in force in the deeper-water sediments of the period, a well-defined faunal change indicative of the attainment of an important evolutional stage, of world-wide significance, and independent so far as can be determined of any change in the nature of the sedimentation. This appears also to be the horizon of the entrance in force of the true Pentamerids (*Barrandella*) amongst the faunas of shallow-water type.

With regard to these general principles of modern classification, there would appear also to be only one really effective way of rescuing our Science from the increasing burden of local nomenclature; this has had its uses undeniably in indicating the exact nature of local successions and developments, and at present cannot be avoided in unfossiliferous rocks such as those of Pre-Cambrian age, but for the rest of our Lower Palæozoic rocks surely the time is coming, if, indeed, it has not already come, when there may be detected emerging from all this wealth of local detail a general palæontological sequence that may be of wide and possibly even of international application.

There will, no doubt, be those who will object on the grounds that in adopting such a classification the geological world might be at the mercy of the whims of a few palæontologists; this should not be the case if the evolutionary principle be adopted, for the choice of fossil indices should then be limited to those fossils that are of the nature of stable or successful types, for these are likely to be the only forms with a sufficiently wide distribution in space to be really useful, whilst in widely remote areas, should these fail, corresponding forms at a similar general stage of evolution should be utilised in their stead.

The most reliable classification for shallow-water beds would then

be that based upon the evolutional sequence of the members of one or more species-groups [5] belonging to genera possessing considerable possibilities of variation (variation gradient) so long as such members continue to be important and characteristic members of the fauna; when they fail in this respect they should be replaced by the members of another species-group that succeeds in importance. Thus, as will be shown in the sequel, the evolutional series comprised within the species-group of *Calymene cambrensis* might well be adopted for classifying the lower part of the Ordovician in our own country, whilst in the upper part members of the species-group of *C. blumenbachi* might be utilised. It may prove advisable in some cases to choose genera belonging to two distinct phyla to serve as a check upon each other such as, in the case of certain of the Lower Palæozoic rocks, might be afforded by Trilobita and Brachiopoda; in other cases species-groups of either of these phyla might prove sufficient.

Deeper-water Faunas of the Lower Palæozoic.

We must now pass on to the consideration of some Lower Palæozoic Faunas, and see what has been achieved by regarding them from the evolutional standpoint; and since, for reasons already given, it would seem that the faunas of the deeper waters must be taken as the standard for purposes of classification, these will be considered first.

Throughout the greater part of Lower Palæozoic time the Graptolite Shales constitute the typical deposit of the deeper waters of our epi-continental seas, the factors controlling their accumulation being not depth as such, but rather the factors that are closely associated with depth, especially quietness of the water, and absence of coarse sediment. Strictly speaking, I suppose the graptolite fauna does not belong to the Black Shale, since it is in all probability pseudo-planktonic; but owing to similar conditions governing its distribution the two are almost invariably associated and may be taken in that sense to belong; in any case, its occurrence is independent of those factors which make for heterogeneity in the faunas of the shallower waters, so that the Graptolite Shales furnish the standard sequence for purposes of classification. As regards the study of this highly interesting group of organisms, it is as well to note at the outset their extraordinarily favourable position from the evolutionary standpoint; for though we may not know the complete story of the whole class of the Graptolithina, since at present its actual beginning is uncertain, we do appear, at any rate, to have a more or less complete history of the more important order, the *Graptoloidea*, comprised within the rocks of Lower Palæozoic age; so that here, if anywhere, we ought to be able to study the various forms in their true relationship to each other. To a large extent this can be done, and the honour of its first conception belongs to Nicholson and

[5] *Species-group* or gens may be considered to be the aggregate of all the species which possess in common a large number of essential properties and are continuously related in space or time. Vaughan, *Q.J.G.S.*, 1905, vol. lxi, p. 183.

Marr, who in 1895[6] pointed out the evolutional importance of the simplification of branching; the work is still far from complete, though more lines of evolutional importance have been added to that of Nicholson and Marr. At the present day we can study the lines along which general development took place, see how different species-groups arose, reached their acme, and diminished in importance as they were succeeded by those of the next evolutional stage; and we can note the horizons at which the more important of these evolutional stages were reached.

Looked at purely from the evolutional standpoint there seem to be at least three main lines along which the graptolites evolved as a whole:—

1. Change in direction of growth.
2. Simplification in branching.
3. Elaboration of cell type.

The first of these brings about a change from the primitive pendent or hanging form to the scandent or climbing position, and appears to be brought about by the necessity for the better protection of the nema or attachment organ, and would, therefore, seem to arise in direct response to environment.

The second line of development eventually results in reduction of the total number of stipes or branches of the rhabdosoma to one; the earliest attempt in this direction, where the tendency to reduction outdistances that of change in position of growth, appears to be unsuccessful, since the forms are all 'dead ends' undergoing apparently no further development of any kind (*Azygograptus*). The later attempt is combined with further change in the position of growth, so that the forms which result are scandent one-branched graptolites with a well-protected nema; these are obviously highly successful, undergoing a rapid development in many different directions.

This simplification in branching may, as suggested by Nicholson and Marr, be the impression of the struggle for an adequate food supply.

The third line also occurs in what may be termed two episodes, and is of a somewhat different nature each time; the earliest elaboration affects the cell as a whole, whereby the cell with a bend or sigmoid curve in it is gradually evolved from a straight tubular cell, the curvature eventually becoming so pronounced that there is torsion of the whole apertural region. Since the development of a cell of this type would allow of closer packing, its evolution, like that of the simplification in branching, may be the impression of the struggle for food; if so this type of cell elaboration may result in response to conditions of environment.

In the second episode the elaboration is of a totally different nature, and seemingly results as the expression of two definite tendencies or trends within the organism, one a trend towards *lobation*, the other a trend towards *isolation*.

So it comes about that the broad outline of the Graptolite History is found to be comprised within four chapters, all dealing with different

[6] 1895, Nicholson and Marr, 'Phylogeny of the Graptolites.' *Geol. Mag.*, dec. 4, vol. ii.

evolutional stages, each chapter being capable of further divisions into sections and sub-sections.

The four chapters of the story may be summarised as follows:—

1. General simplification of branching coupled with change in direction of growth. Attainment of unsuccessful one-branched form, which undergoes no further development. Characteristic of Arenigian and Llanvirnian beds.

2. Commencement of elaboration of cell type (first episode). Characteristic of Llandilian beds.

3. Widespread attainment of the scandent position by two-branched forms. Characteristic of Caradocian and Ashgillian beds.

4. Widespread attainment of one-branched stage by scandent forms which undergo conspicuous elaboration of cell type (second episode). Characteristic of the whole of the Silurian.

The first chapter deals mainly with the many-branched graptolites, of which the best known is the 8-branched form *Dichograptus*, and the most obvious changes shown are those tending in the direction of the reduction of number of stipes or branches. Thus the 32-stiped forms are gradually succeeded in time by those with 16 stipes (*Loganograptus*), the 16-stiped by those with 8 (*Dichograptus*), the 8 by 4 (*Tetragraptus*), and the 4 by 2-branched forms (*Didymograptus*). Thus the simpler forms succeed the more complex, and *at the same time* there is a gradual change from the pendent through the horizontal to the scandent position of growth; by the time this is attained the number of stipes is reduced to four, so that such forms are essentially scandent or climbing forms of *Tetragraptus*, though they are more familiar under the name of *Phyllograptus*.

This, the first attainment of the scandent position of growth, is an evolutional stage of considerable significance, and differentiates the upper part of this first or DICHOGRAPTID fauna from the lower containing the many-branched graptolites; the 2-branched horizontal *Didymograpti* become abundant at the same horizon (*zone of D. extensus*), so that it is easy of recognition without any knowledge of graptolite species. The *Phyllograptus* stage is short; there is a certain degree of elaboration, and then further reduction in the number of stipes to two follows on, and the place of *Phyllograptus* is gradually taken by *Glossograptus*, a graptolite common for the first time in the Llanvirn rocks, and often a conspicuous element in the faunal assemblages of that age. The structure of the proximal end of these two graptolites is so peculiar and so alike that there can be no doubt of their relationship; moreover, I regard the septal spines of *Glossograptus* as possibly representing the last vestiges of the thecæ of the third and fourth stipes of *Phyllograptus*. Structural resemblances of such a kind may naturally be made out in the laboratory or museum, but the realisation of the true connection between them and their proper place in the evolutional line only becomes obvious when they are seen gradually replacing each other in the field with all the intermediate stages.

If now simplification in branching be accepted as a line of evolution, how does it come about that many-branched graptolites are often found occurring on the same slabs of rock as those with four or even with

only two branches? Field evidence supplies the answer to this very natural query. Whilst the earliest graptolite with which we are acquainted was a pendent form, there very quickly followed other graptolites in which a horizontal direction of growth replaced the earlier pendent direction, though both occur side by side in rocks of the same age; the horizontal growing forms we term *Clonograptus*, the pendent *Bryograptus*. Now it is perfectly obvious from observation in the field that as regards simplification of branching the same plan of evolution was followed in both these groups, though there is always a tendency for development to lag behind and go slower in the pendent line, whereas development is so rapid in the horizontal line that many *Clonograpti* persist alongside the 8-branched *Dichograpti*, though when *Dichograpti* persist alongside the 4-branched *Tetragrapti* they are most commonly those in which a certain amount of simplification has already taken place, since they are, as a rule, forms with only six or five stipes instead of eight; owing, however, to the unequal rate of development in the two groups there is a characteristic association of *pendent* 4-branched graptolites with *horizontal* 2-branched forms of the type of *D. extensus*, whilst *horizontal* 4-branched forms have become rare.

The apparent anomaly is thus clear when followed out step by step. This greater rapidity of development in one group seems to indicate that these horizontal-growing forms were the more successful of the two; and it is, therefore, perhaps only to be expected that almost all the later graptolites are developed from ancestors within that group, the exceptions being those whose ancestry is at present obscure, but there is no indication that these arose from any member of the pendent group; I have so far been utterly unable to find any graptolites in later beds which seem to be connected with these. If I am right in supposing that the change in direction of growth of the rhabdosoma was connected with the protection of the hollow thread-like nema (*virgula auctorum*), which is the attachment organ so vitally necessary to the colonial organism, the forms belonging to the pendent group may be regarded as unsuccessful because they fail utterly to secure this necessary protection, and, therefore, the members of this group come entirely to an end at the top of the Llanvirnian. Within the other group protection is better achieved, since in many cases at any rate the horizontal-growing stipes appear to have been plastered on to foreign bodies or suspended therefrom by short threads, and the nema would, therefore, in most cases have been short. Within this horizontal group the goal in simplification would seem to have been reached early in the one-stiped *Azygograptus* of the Middle Arenig; this type is repeated more than once at slightly higher horizons, but appears to be in no case a successful form; individuals are very commonly broken in the region of the sicula, which is in itself suggestive, and they, like the pendent *Didymograpti*, appear to be 'dead ends.' The successful one-stiped form is attained much later by very devious routes through the scandent or climbing graptolites, and in all of them the attachment organ is very perfectly protected, partly by being buried within the rhabdosoma for the whole of its initial region, and partly by the development of a special encasing tube or sheath.

Practically all the graptolites referred to above, which are the predominating element in the fauna, are characterised by simple cells—*i.e.* at most a reproduction of the embryonic sicula slightly modified in respect of relative length and breadth—and they follow what I have called elsewhere the Dichograptus plan of development; they may, therefore, be regarded as constituting the first or Dichograptid Fauna, which is pre-eminently characteristic of the rocks of Arenigian or Llanvirnian age. Without any special knowledge of species or genera, the horizon of this fauna may be recognised by the presence of branched graptolites with simple thecæ, the presence of scandent forms being indicative of the higher beds.

In all the earlier graptolites, as has been shown, the cell type is simple, but soon after the two-stiped horizontal *Didymograpti* have developed a slight change begins to be apparent in the thecæ of some forms; this shows itself in a drawn-out curvature of the cell wall and a turning in of the apertural margin, which gives a most striking and characteristic appearance to the cell after compression. This is first apparent in the thecæ in the region of the sicula, and becomes less conspicuous as the stipe grows in length; for it may be noted at this point that all progressive development (anagenesis) is first indicated in the proximal and, therefore, youthful region of the rhabdosoma, and when retrogression (catagenesis) occurs, it is in this same proximal region that signs of former elaboration are retained.

Throughout the earlier rocks of Llandilian age the great majority of the graptolites have cells of this slightly elaborated type and two stipes only, which are reclined or reflexed in their position of growth; but gradually in some forms an increasing degree of curvature of the walls of the cells becomes apparent, and the incurving of the apertural region is accompanied by a degree of torsion that after compression causes a very different appearance according to whether the rhabdosoma is viewed from the front (obverse) or back (reverse). This is the *Dicellograptus* stage, and so distinct is the appearance of this graptolite from any *Didymograptus* that it would never be considered related if the successive stages had not been followed step by step. It may be noted, too, that whilst this cell elaboration is in progress evolution along other lines seems to be temporarily arrested, but is resumed when the elaboration has reached its acme, especially towards the attainment of the scandent position of growth; this is at first only partial, as in *Dicranograptus*, but is eventually complete, as in the closely related *Climacograpti*, which are scandent throughout. The relationship of *Climacograptus* is clearly with *Dicranograptus* and *Dicellograptus* rather than with *Diplograptus*, with which up to the present it has been invariably grouped. The only connection it really has with *Diplograptus* is that, being a biserial scandent form, it is at *a similar evolutional stage*.

Since the simpler type of thecal elaboration is characteristic of the graptolite *Leptograptus*, the various forms in which this type of theca is found may suitably be regarded as constituting the second or Leptograptid Fauna, and its occurrence, whether in simpler or more complex forms, may be taken as indicating rocks of Llandilian or Caradocian age. As will be shown later, other features more particularly charac-

teristic of the Caradocian will serve readily as a guide to discriminate between these two, whilst the degree of elaboration shown will afford some indication as to whether the lower or upper part of the Llandilian is indicated.

There is no new element definitely to be associated with the third chapter of the graptolite story, and yet perhaps the opening paragraphs are as striking as anything in the whole narrative. A feature that cannot fail to arrest the attention of every field worker is surely that extraordinary development of large *Diplograpti* and *Climacograpti* that characterises the junction of the Llandilian and Caradocian rocks. So far as can be determined from field evidence this swarm of *Diplograpti*, particularly of *Orthograptus* type, is due to development along at least two lines reaching their acme at approximately the same time, and when these meet the *Climacograptus* lines the result is bound to be very striking; moreover, since most of these are clearly highly successful forms, giving origin to numerous varietal modifications, the predominance of the scandent biserial graptolites is pre-eminently the distinctive feature of the rocks at this horizon. Hence the various associated graptolites may be regarded as belonging to the third or DIPLOGRAPTID FAUNA.

In the lower beds belonging to this fauna the complex-celled *Dicellograpti* and *Dicranograpti* still persist, but the association of the large *Orthograpti* is sufficient to differentiate the horizon from the Llandilian. This is the association characteristic of the Caradocian.

So far as the graptolite faunas are concerned there is obviously a close connection between the Caradocian and the Ashgillian, the predominance of the *Orthograpti* continuing to be a characteristic feature; in the lower beds regarded as Ashgillian there is some evidence of retrogression as respects the *Dicellograpti* and *Climacograpti*, both showing a return to the simpler type of cell; the stages of this, however, have not as yet been completely worked out. The highest beds, which from the point of view of their graptolites should logically be grouped with this third fauna, include some at present very generally grouped with the Silurian. In these *Diplograpti* (*Orthograpti*) and *Climacograpti* are still predominant, though the *Dicellograpti* have disappeared.

The next striking feature is the coming in of *Monograptus*, or, if expressed evolutionally, the uniserial (one stiped) scandent graptolite, a very important and easily recognised evolutional stage. This marks the successful attainment of the end along two lines of development: simplification in branching, and change in direction of growth. It is pre-eminently characteristic of Silurian rocks.

In the earliest graptolites reaching this stage there is nothing new as respects the cells; all are 'old-fashioned' types seen previously in *Diplograptus*, *Climacograptus*, *Leptograptus*, or *Dicellograptus*; but with the attainment of the uniserial scandent form the organism seems to have had its energies set free to follow further trends, these being in the main in the direction either of lobation or isolation, but they do not keep quite apart; a certain degree of lobation creeps into the line of isolation, and a certain amount of isolation is clearly discernible in the

lobate line; nevertheless, one or other trend is always the more conspicuous and the more definitely followed. In both these lines the trend continues to the point where, as Lang[7] has so ably described it, 'their exaggeration puts the organism so much out of harmony with its environment as to cause extinction'; the lobation is developed till the aperture of the cell is practically closed (*Monog. lobiferus*), and isolation is carried to such a pitch that the cells seem readily to have fallen apart from each other altogether, so extremely slender is the connecting portion (*Rastrites maximus*). The hooked variant of the lobate line, however, fares much better, and can be seen to work steadily up to its acme (*M. priodon*), and as steadily decline until the cell-form is seen to have returned to the point from which it started.

These are the general facts concerning the evolution of the group as a whole. We may now see the way this works out along a few particular lines.

1. *Bryograptus to Didymog. indentus.*
2. *Clonograptus to Didymog. hirundo.*
3. *Monog. cyphus to Monog. tumescens.*

In the first of these lines the evolution is purely in the direction of simplification in branching, the thecæ being practically identical throughout and the pendent position of growth unchanged. Thus we pass successively from *Bryograptus kjerulfi* to *Tetragraptus pendens* by failure of branching, and thence to the two-branched *Didymog. nanus*, which by slight modification seems to pass into the form known as *D. indentus*; this is really only a late mutation[8] of *D. nanus*. In the second case there is simplification of branching combined with change in direction of growth and some increase in the size of the thecæ. The first conspicuous change is the change in position of growth from pendent to horizontal, resulting in *Clonograptus flexilis*, a 32-stiped graptolite, thence by gradual stages to *Loganograptus logani*, a 16-branched form, and by further reduction in the number of branches to 12, 11, 10, and 9 an important stage is reached in the well-known 8-branched form *Dichograptus octobrachiatus*. This passes successively through what may be termed septad, hexad, and pentad stages before attaining another important stage, that of the 4-branched form *Tetragraptus quadribrachiatus*, a horizontal form with perfect symmetry. It may be noted that the commoner and more widespread forms are always those in which there is symmetry. Now such a form as *T. quadribrachiatus* has two obvious lines of variation: it may continue the process of simplification in branching or it may change its position of growth. It appears to do both, and so two lines diverge at this point with very far-reaching results.

(*a*) follows the tendency for change in position of growth, and passing through the reclined forms *Tetrag. amii* and *T. serra* leads into that scandent *Tetragraptus* which we know better as *Phyllograptus*, the earliest scandent graptolite, and a very important form indeed; for

[7] 1923, Lang. 'Evolution; a Resultant.' *Proc. Geol. Ass.*, 1923, vol. xxxiv, p. 11.

[8] Mutation.—This term is used throughout in Waagen's sense and not in that of De Vries.

simplification in branching follows whereby the stipes are reduced to two, and an entirely new factor supervening in localisation of thickening in the graptolite wall, the line diverges in one direction and leads into the Retiolitidæ, a quite distinct species-group.

(*b*) follows the tendency to simplification, and passes into the two-stiped form *Didymog. extensus*, and thence to an unsuccessful one-stiped graptolite *Azygog. eivionicus*. The two-stiped form undergoes also various modifications in the packing of the cells, and passes through *Didymog. nitidus*, a very variable graptolite, into *D. hirundo*, a more stable form, which is, however, apparently a dead end. In others of this *Didymog. extensus* type an actual modification of the cell structure supervenes as an entirely new factor, so that the cell, instead of being a simple tube, is gradually bent and twisted and its aperture turned in. The details of this have yet to be worked out completely, but the general plan is perfectly clear, and leads first into the *Leptograpti*, and thence into the *Dicellograpti*, *Dicranograpti*, and *Climacograpti* in turn.

Lastly, we may study the elaboration of the cell as seen in the second episode, the evolution of the hooked variant of the apertural lobe. Here, starting from *Monog. cyphus*, which has the old-fashioned Dichograptus type of cell, we find the first traces of a hook in the closely related *M. revolutus*, and can trace its gradual development in the proximal region of the rhabdosoma in *M. difformis* and *M. argenteus*, in which, though the hook is well developed proximally, the distal thecæ are still simple; gradually the hook-form invades the whole rhabdosoma (*M. clingani* and *M. sedgwicki*), and taking on its most distinctive features in *M. marri*, reaches its acme in *M. priodon*, perhaps one of the best-known graptolites all over the world. Thereafter retrogression sets in, the hook becomes less pronounced in *M. flemingii* s.s., and a small, highly characteristic variety is seen occurring side by side with the larger form; this smaller variety gradually gives way to *Monog. colonus*, in which only the proximal thecæ retain any signs of their former elaboration, and *M. colonus* itself is replaced by *Monog. tumescens*, where all thecæ are once more of the unhooked type just as in *Monog. cyphus*, though the form of the rhabdosoma of *M. colonus* is retained. This is one of the latest graptolites with which we are acquainted.

Shallow-water Faunas of the Lower Palæozoic.

The case of the shallow-water faunas of Lower Palæozoic age must now be considered; and here, in spite of a vast amount of work that has already been accomplished, much remains to be done, but from a different standpoint and along very different lines. There exists already a great mass of more or less purely descriptive literature, accompanied in general by illustrations of varying degrees of merit. All this has a value of its own; it provides descriptions which aid identification of fossils, and in many cases gives an excellent idea of the variety of the brachiopods, trilobites, or corals represented in a certain bed or set of beds; but, looked at broadly, is not its value to a great extent purely numerical, giving an idea mainly of the relative abundance of certain

fossils at certain horizons and their relative scarcity at others? Since such work has too often unfortunately been carried out in the museum or laboratory by workers unacquainted with the fossils in their natural environment, it is liable to fail to take note of peculiarities of preservation and condition that may be significant, and new names have in the past been sometimes given to the same fossil in different conditions of preservation, or to other forms which owe their apparent peculiarities to the deformation of the rocks in which they lie. As is perfectly well known, the older rocks of this country have almost always suffered more or less considerably in this respect, though in the case of some rocks, such as mudstones, it is exceedingly difficult to estimate the degree of such deformation in hand specimens removed from their proper surroundings. So, too, the relative sizes of fossils may take on a totally new aspect when seen in the field. In such a connection we may note the characters of the Caradocian faunas of Shropshire and North Wales respectively. Similar fossils from the two areas differ so much in size that the existence of small Welsh varieties is inevitably suggested, until it is realised when the faunas are seen in the field that the *whole* Welsh fauna is of smaller size though otherwise very similar, therefore obviously we are here dealing not with any true varieties but rather with a whole fauna living under less favourable conditions.

The pity of it is that, in spite of all the labour and skill that has been expended, we are still left so largely in ignorance of the crucial facts that in these days we want to know. There is a very real need at the present time for the co-ordination of these descriptions so far as possible on genetic lines. The difference between the past and future palæontological work appears to me to be just this: the older type of work is too dead, whilst the palæontology of the future must be essentially alive; it must vitalise fossil organisms, and regard them as parts of once-living entities possessing definite ancestors and descendants. developing along definite lines which are the result partly of internal and partly of external forces.[9] The biologist will find his interest in the degree of relationship between species-group and species-group, or in the precise relationship between ancestor and descendants within the species-group, but the value of the work to the geologist will lie rather in the determination of the definite lines along which evolution takes place and the horizons at which important and easily recognised evolutional stages are reached.

It may perhaps be argued that the geological record is so imperfect that our story can at the best be of little value, because it will be so incomplete; to that I would reply that such features as have been sufficiently permanent in any organism to impress themselves upon the hard parts that are all that remain to us are likely to be those of enduring significance, and therefore particularly reliable so far as they go. We may miss detail, but the main facts of the story should be beyond question. Up to the present time resemblances and differences existing between certain fossils have often been noticed as points to render identification more accurate, but their true significance has too

[9] Lang, *loc. cit.*

often been missed. Classifications have also been given claiming to be genetic, but too often all that has been done has been the placing in the same group or class, forms that have reached *a parallel evolutional stage*, and since many of the more conspicuous evolutional stages appear to be reached at approximately the same time, even though along different lines, such a classification is chronological rather than biological. From the geological standpoint a chronological classification is valuable, but the biological side must not be ignored. Thus we have seen in the classification of the Graptoloidea, *Climacograptus* and *Diplograptus* are both included in the family of the *Diplograptidæ*, presumably because they are both biserial and have both attained the scandent position of growth; they have no other connection and appear to have totally different lines of descent. The same is to a large extent true of Pompeckj's classification of the Calymenes.[10] Thus Pompeckj divides the Calymenes proper into two sub-genera, *Pharostoma* and *Calymene*. The forms included under the s.g. *Pharostoma* are stated to be characterised by the presence of long genal spines and the termination of the facial suture at the posterior margin. These two are closely connected, for the presence of genal spines seems to inhibit the facial suture coming out at the genal angle as in the s.g. Calymene; hence if, as Pompeckj himself suggests, the possession of spines is a primitive character, it carries with it a notable stage in the development of the facial suture, since until the spines have disappeared the facial suture cannot come out at the genal angle. Hence the rounding of the angle and the position of the termination of the facial suture together mark an evolutional stage that is regarded as characteristic of the s.g. *Calymene*. He also places *Calymenes* of the *tristani* type in a totally different section from the *Calymenes* of the type of *C. cambrensis* (*Calymene* s.s.), for he holds that the lobing of the glabella is so different that 'relationship is not to be thought of,' whereas I hope to be able to show that, looked at evolutionally, these forms may be regarded as belonging to different points along a special trend line, that of evolution of the glabella lobes, and the appearance of bifurcation in the glabella furrows upon which he lays such stress as a feature of importance in classification appears to me to be a necessary stage in the lobal evolution, and therefore only highly developed at a certain stage.

On the other hand, *Calymene caractaci*, which he places in the same group as *C. cambrensis*, apparently chiefly on the grounds of the course of the facial suture and number of glabella lobes, does not appear to me to be so closely related from the genetic point of view, since these two differ markedly in other characters that must, I think, be considered 'essential,' and therefore belong more likely to different species-groups.

A glance at the table given at the end of his paper will serve to show how largely this classification is chronological. It is probably true that the greater number of our fossil 'genera' at the present day are polyphyletic, and cut across true lines of evolution, as can be demonstrated

[10] 1898. Pompeckj, J. F. 'On Calymene Brongniarti.' *Jahrb. f. Mineral., Geol. & Pal.*, vol. i, p. 187.

in the case of the Corals, Trilobites, and Graptolites. The true relationship existing between individual fossils and fossil-groups will probably only become manifest after searching examination in the field, and whilst many of the species previously established will no doubt stand, others will probably be found to be more truly related to certain central forms as space or time variants (mutations), and may or may not be worth specific rank. So that the evolutional work that is required must be carried out primarily in the field, though supplementary work will have to be carried out in the museum or laboratory; but the value of different features can, I believe, be only truly estimated when they are seen making their first appearance, gradually coming to their acme, and then dying away to be replaced by others. Thus we may study in the field all the stages between fossil A and fossil B, whose relationship to A would probably otherwise never have been suspected, so different do the two extreme types appear. It was indeed truly said by your President three years ago[11] 'that not until we have linked species into lineages can we group them into genera, not until we have unravelled the strands by which genus is connected with genus can we draw the limits of families, not until that has been accomplished can we see how lines of descent diverge or converge so as to warrant the establishment of orders.' This is equally applicable to shallow- and deeper-water faunas alike, but the time and space variants are best seen in shallow-water faunas, where the variation gradient being spread out over thicker deposits is less steep than it is in the deeper-water faunas, where it is often so steep that the time-variants tend to become absorbed in genera.

The facts just dealt with concern the more purely biological side of the question, but for the geologist there is more in the evolutional method of work than this. Bearing in mind that Palæontology fulfils one of its chief functions as the handmaid or helper of Stratigraphy, we may ask how far evolutional work will accomplish that object. The answer is clear and definite. The Lower Palæozoic faunas, as has already been stated, are essentially Brachiopod-Trilobite faunas together with Corals where the seas were sufficiently clear to permit of their growth and development.

As regards the Corals, the kind of work required is that initiated by Vaughan, and most ably extended by Dixon, Carruthers, Stanley Smith, and others. Lang[12] has recently performed splendid service in the cause of evolutional palæontology in putting forward his Doctrine of Trends, and showing how Carboniferous Corals follow what he terms *Programme Evolution*, since coral stocks continually developed along parallel lines so that different lineages may go through the same sequence of changes. We may hope that some such trends may be discernible amongst the corals of Lower Palæozoic age, and Carruthers[13] has shown us how best to obtain the knowledge we require. In

[11] 1920, Bather, F. A. Pres. Address to Section C., Cardiff.

[12] 1923, Lang, W. D. 'Trends in British Carbonif. Corals.' *Proc. Geol. Ass.*, vol. xxxiv, pt. 2, p. 120.

[13] 1910, Carruthers, R. G. 'Evolution of *Zaphrentis delanouei.*' *Q.J.G.S.*, vol. lxvi, p. 523, &c.

his most admirable account of the evolution of *Zaphrentis delanouei* Carruthers has shown the importance of cutting serial sections, for the stages seen in the adult of early forms are often characteristic of adolescence in forms at higher horizons. Thus in *Z. delanouei* evolutionary stages are confined to the shape of the cardinal fossula and the length of the major septa, and different time-variants (mutations, Waagen) show striking differences between these.

In *Z. delanouei* s.s., which occurs in the Cementstone Group 300-400 ft. below the base of the Fells Sandstone, the transverse sections show septa meeting in the centre of the corallum and a large cardinal fossula expanded towards the inner end; together with this form there occur others which agree with *Z. delanouei* in their adolescent stage, but in the adult a stage is reached in which *the walls of the fossula become parallel* and finally show a tendency to constriction at the inner end. Since this mutation marks an important evolutional change as regards the fossula, it is termed *Z. parallela.*

At a considerably higher horizon, in the Lower Limestone Group, the cutting of sections of a fresh mutation foreshadowed in the Cementstones reveals no trace remaining of what may be termed the *delanouei stage;* but the *parallela stage* is distinct, and with growth the inner end of the fossula narrows, whilst in sections of the adult stage the constriction becomes very pronounced, the septa being, however, still joined together in the centre of the corallum. Again, on account of a further change in the character of the fossula this mutation may be distinguished as *Z. constricta.* Within the Lower Limestone Group are also found forms representing a further change; these do not pass through the *parallela stage*, but start at the *constricta stage*, and on further growth the septa shorten until they separate at the centre of the corallum. This again is an important and easily recognised stage (*Z. disjuncta*), and this mutation is said by Carruthers to show amplexoid characters (=amplexoid trend, Lang). The geological value of these changes lies mainly in the fact that they are continuous in time and characteristic of different stratigraphical horizons, apart from whether they are progressive or retrogressive, but it is clear that careful discrimination may at times have to be made between these.

At the mere thought of coping with the many evolutional problems connected with the Lower Palæozoic Brachiopods the heart of the most vigorous palæontologist amongst us might well fail him. I suppose that there is no single worker on the Lower Palæozoic rocks who has not at one time or another realised the stupendous nature of the problem that awaits us here. We have, I feel sure, all been conscious of the fact that many of the so-called long-ranged species are not really quite the same, but show certain differences at different horizons with which in the course of our field-work we have become familiar and can recognise, so that for the sake of our own convenience we have often given them the field-names; but when we try to analyse these differences palæontologically each character seems so slight as to be trivial and unimportant; nevertheless, in bulk they may be important and the two extremes quite distinct. This may well be illustrated by the case of the Dalmanellas as represented by the species *D. elegantula*, a name

which as at present used does not define a species but an important *species-group*, the earliest members of which occurring low down in the Ordovician are certainly markedly different even as regards the external ribbing of the shell from those occurring at the base of the Silurian, though all have been included in the same diagnosis. Up to the present all we can do in naming such a fossil is to term it, in despair, *Dalmanella of elegantula* type.

This work can and must be tackled group by group; it will demand an amount of careful field-collecting, in the first place, of specimens showing internal as well as external characters, for these last are by no means to be neglected, since they often reflect changes in internal characters, though they do not do so invariably; hence it will be necessary to distinguish between those possessing different internal and external characters and those which differing in their internal characters yet may have the same external characters.

Field palæontology, when it has a definite aim of this sort in view, becomes a fascinating and absorbing study, and a fresh zest is given to the somewhat monotonous task of mere fossil-collecting.

Kiaer, in his classic memoir on the Silurian Rocks of the Christiania Basin,[14] has indicated to us how this work may be carried on. He was fortunate in that the rocks in the area where he did his work are but slightly inclined and are affected only by faulting and not by folding, so that there can be no doubt as to the order of succession of the various beds. To a large extent Kiaer has applied the principles of evolutional palæontology with great success; he notes the appearance of early mutations and their gradual evolution at successive horizons up to and beyond the development of the typical form. Thus he utilises the evolution of the septum in the Pentamerids of the species group of *P. oblongus*; he notes how this septum is short in *Barrandella undata*, the earliest of the true Pentamerids, and shows how this gives place upward to another mutation, *P. borealis*, with a septum which, though rather longer, is nevertheless shorter than that of *P. oblongus* s.s., which is next developed. At a still definitely higher horizon is found *P. gotlandicus*, probably to be regarded as a late mutation of *P. oblongus*, in which the septum is still further developed.

Having arranged these Pentamerids in order, Kiaer is able to throw light on the development and relation of the Stricklandinias, among which there has been and still is much confusion in this country. He shows that *Stricklandinia lens* makes its appearance in the Christiania Basin with the *borealis* mutation of *P. oblongus*, and is followed at a slightly higher horizon by a mutation of its own, whereas *S. lirata* does not occur till the horizon of the *galeatus* mutation.

For purposes of correlation, however, Kiaer notes the position of the beds containing the fossils in relation to the deeper-water Graptolite Shales. Thus, for example, beneath his zone of *Barrandella undata* he recognises the zone of *Cl. normalis*, the equivalent of our British zone of *Diplog. acuminatus*, and some little way above his zone of *Pentamerus oblongus* he notes the graptolite zone of *Cyrtog. Murchisoni*,

[14] 1908, Kiaer, J. 'Das Obersilur im Kristianiagebiet.'

and taking that rightly as representing the base of the Wenlock, he concludes that all the zones of 'shelly' beds in between must belong to the Valentian.

In the course of work amongst the rocks of Ordovician age I have been struck with distinct evolutional trends amongst some of the commoner trilobites, the stages of which have proved valuable as indices of age. In illustration of this I may quote two:—

1. The evolution of the glabella lobes in a species-group of *Calymene*.
2. The relation between the segments of the side lobes and axis in the pygidia of *Encrinurus*.

With regard to the first of these, the evolution of the lobe, two things have to be noted:—

(*a*) The number of the lobes.

(*b*) Their character—*i.e.* the degree of rounding off into a real lobe.

The number of lobes appears to increase steadily in proceeding from older to newer beds; thus, for example, Silurian forms in general have more lobes than those of Ordovician age. The actual character of the lobe is to a large extent determined by the state of development, both as regards depth and breadth, of the curved glabella furrows. Primarily the lobation seems to arise as the necessary result of the development of such curvature; the glabella furrows appear to develop gradually in width from above downwards, and at the same time increase in breadth; the lobation of the basal lobe, for example, is complete when the downward curvature of the first furrow cuts into the upward curvature of the neck furrow, and the furrow is deep and broad throughout its extent; but before this stage is attained there are many degrees in the development from an incompletely developed furrow through one where, though more or less complete, it is still so shallow for a part of its course that the lobe is not cut off, but appears definitely attached to the rest of the glabella by a 'neck' or bridge.

The Calymenidæ appear in part at any rate to be derived from the Olenidæ, and starting with the earliest known *Calymene* occurring in our British rocks of Ordovician age we may note that the general form of the glabella is still definitely oval or parabolic in outline, the neck furrow incompletely developed, and the two glabella furrows fairly deep but short and oblique, giving more the idea of indenting the general outline of the glabella than of cutting off a lobe; the outer edge of the segment too, being still that of the outline of the glabella, is straight; there is, moreover, at this stage no very conspicuous difference in size between the two segments, though there is a tendency for the posterior pair to be slightly the larger of the two. This is the form known as *Calymene tristani*, which is characteristic of the trilobitic beds immediately below and associated with the graptolite zone of *Didymog. extensus*. At a slightly higher horizon, that of the graptolite zone of *Didymog. hirundo*, there is found a similar form hardly to be distinguished from *C. tristani* except for the *greater distinction of the basal lobe* and the curvature of the second pair of glabella furrows (*C. parvifrons*), whilst another type with a less parabolic glabella more truncated in front makes its first appearance (var. *Murchisoni*). Within the Ordovician up to this horizon, despite various descriptions hinting the

contrary, I have never observed any *Calymene* which had any indication of more than two glabella segments, but in higher beds the equivalents of the zone of *Didymog. bifidus* there may be detected in some forms, otherwise very closely allied to the *C. parvifrons* of the horizon of the zone of *Didymog. hirundo*, the occasional presence of a third glabella furrow; this is, however, always obscure, and its presence is generally accompanied by a very definite difference in size in the glabella segments, largely induced by the increase in breadth of the furrows, the basal segment at this stage being very decidedly the larger. By the time the horizon of the Llandilo Limestone is reached (zone of *Didymog. Murchisoni*) this third lobe, minute though it be, is constant and perfectly definite in form; also the proximal pair of glabella furrows are now curved to such an extent that the basal segment may be regarded as constituting a pair of basal lobes; the curved furrow is, however, so shallow for part of its course in the middle that there is still a distinct 'neck of attachment.' The so-called bifurcation of the glabella furrow, to which much importance has been attached in classification, seems to arise as a direct consequence of this tendency to lobation; the lobation of the basal segment is not, however, yet complete; there is still some angularity on the outer side, the oval parabolic outline of the glabella as a whole being still obvious.

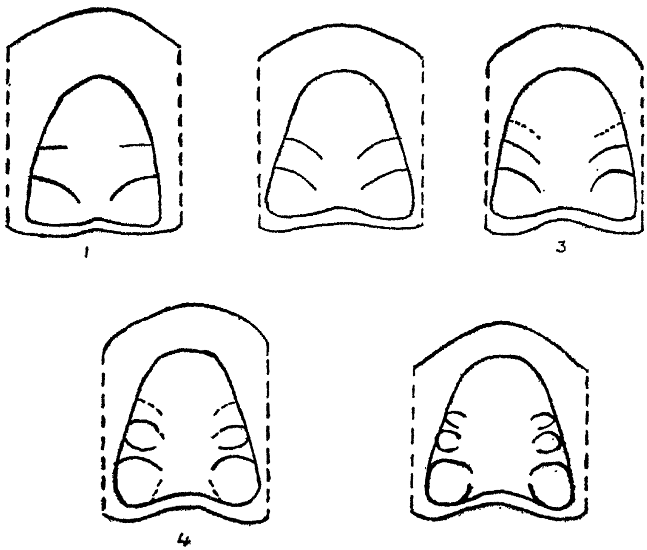
This stage, the development of a basal lobe and the presence of a third segment, seems to mark a definite advance and to constitute a very successful form, for this *Calymene*, *C. cambrensis*, is very stable in its characters in many different kinds of sediment, and has a wide distribution in space. In our own country it is one of the few trilobites found in both the Scotch and Welsh types of the Llandilian.

All the *Calymenes* hitherto dealt with are characterised by the possession of a broad frontal region, which has, however, steadily decreased in size relatively to the glabella, but at this horizon there appear to be two forms to both of which the name *C. cambrensis* seems to have been applied, in one of which there is a far more conspicuous diminution in breadth of the margin than in the other, though both are at the same stage of evolution as respects their glabella lobes. This suggests that the marginal development is going to be a factor of importance, and from what happens later it is clear that this is the case, and when it takes place some retardation may be expected on the older line, either as regards the number of the lobes or as regards the perfection of their development.

In *Calymene planimarginata*, the common Caradocian form which retains its broad margin, further development on the old lines takes place; the third lobe, though still small, becomes more definite; there is marked disparity in size as between segments 1 and 2, both of which are distinctly more lobate in character, having lost to a large extent the angularity of the outer margin, though those specimens characteristic of the lower part of the Caradocian (*alternata beds*) are distinctly less perfectly lobate than those of the higher *chasmops beds*; in the lower beds the third segment, though definite, is not lobate at all, whilst at the higher horizon it is lobate but with a definite neck of attachment. Both these forms of *C. planimarginata* are characteristic of the horizon of the

graptolite zone of *Dicranog. clingani*, but others also occur showing that the line has begun to branch in various directions leading into different species-groups, the details of which have still to be worked out. It is, however, clear, I think, that the state of evolution of the glabella lobes may afford a valuable index of the age of the beds in which it occurs.

Further investigation is required to show how far this parallel evolution takes place at approximately the same time in remote areas in different species-groups. So far as I have investigated the problem it would appear to be broadly true in the case of the deeper-water



1. Calymene tristani. (Zone of D. extensus.)

2. Calymene parvifrons. (Zone of D. hirundo.)

3. Calymene parvifrons mut. (Zone of D. bifidus.)

4. Calymene cambrensis. (Zone of D. Murchisoni.)

5. Calymene planimarginata. (Zone of Dicranog. clingani.)

faunas, for so far as the graptolites are concerned the outstanding stages in evolution are reached in the majority of cases at approximately the same time along many different routes, though there are some exceptions, for which the reason is, however, usually obvious. So far as the *Calymenes* are concerned it is of great interest to note that those of Bohemia, though constituting a different species-group from those found in this country, undergo a precisely similar evolution at the same time; thus *C. arago* of D.1. γ shows a very slight and faint indication of a third lobe with a straight outer edge to the glabella, a precisely similar stage to that of *C. parvifrons* in this country; so, too,

C. parvula of D.d.2. is at a stage of development similar to that of *C. cambrensis*, as is also *C. pulchra* at the same horizon in yet another species-group.

It would, I believe, be perfectly possible to adopt a classification of the whole of the Ordovician based upon the evolutional sequence of the various *Calymenes*.

All these facts illustrate that even from the purely palæontological standpoint much field knowledge is essential if a right conception is to be gained of the true relationship existing between species and species. It appears to be also in the highest degree necessary to view a succession of forms like those I have quoted in order to determine what characters are really of importance in the recognition of species.

Also, when lines of evolution result in the attainment of successful forms, not only do these appear to be numerically abundant, but it would seem also that they have a wide distribution in space.

So much, then, for a possible line of evolution in the head of a trilobite; we may next consider the evolution of the pygidium in a very different form. An interesting study of this appears to be afforded by the species-group of *Encrinurus punctatus*. As is well known in the commonest type of this trilobite occurring in the Wenlock Limestone of Dudley, the axis of the pygidium shows a far greater degree of segmentation than do the lateral lobes; this may be interpreted as implying that numerous segments have been incorporated into the tail with a greater degree of fusion in the side lobes than in the axis. The species, moreover, is commonly recognised as possessing two well-marked varieties, var. *arenaceus* and var. *calcareus*, differing chiefly from each other in the possession of a definite mucro in var. *calcareus*, which has been interpreted as being connected with the supply of calcareous matter available, but, viewing the species-group as a whole, it would seem rather to be the natural culmination or acme of a definite tendency to fusion which is developed with increasing persistence throughout its history in time so far as I have been able to study it.

The earliest forms which I have examined are to be found at the horizon of the Stinchar Limestone in Scotland and the Derfel Limestone of Wales. The graptolite shales associated with these limestones prove their age to be Llandilian. At this horizon the relation between the segments of the axis and the lateral lobes of the pygidium never exceeds 2:1, whilst in the two earliest segments the proportion is very clearly 1:1; in the Caradocian the proportion rises to 3:1 for segments 5 and 6, whilst the Ashgillian forms (*multisegmentatus* stage) show 2:1 for segment 2 and still 3:1 for segments 5 and 6. In the Lower Valentian segment 4 has risen to 3:1, whilst in the Upper Valentian it is commonly the third, though there is some variation, since in some cases all that it is possible to make out is that there are five segments in the axis compared with two (2 and 3) in the lateral lobes. In the succeeding Wenlock forms the culmination is reached with 3:1 for segments 2-5, and 4:1 at the sixth; in all these later forms there is a tendency to fusion of the later lateral lobes with the axis, partially, as in the case of 7 and 8, throughout their length, and more definitely at their terminations.

FUSION OF SEGMENTS OF TAIL IN SPECIES-GROUP OF ENCRINURUS PUNCTATUS.

—	LLANDILIAN			CARADOCIAN	ASHGILLIAN			VALENTIAN									SALOPIAN	
Segments on side lobes of pygidium	Derfel Lst.	Stinchar Lst.	Stinchar Lst.	Middle Bala Dent	Slade Beds, Haverfordwest	Slade Beds, Haverfordwest	Drummock Beds, Thraive Glen	Ritton Castle, Shelve	Tortworth	Skelgill	Mulloch Hill Beds, Rough Neuk	Saugh Hill Beds, Woodland Point	Saugh Hill Beds, Newlands	Camregan Beds, Camregan	Up. Llandovery, Penlan	Wenlock Lst., Dudley	Gothland Lst., Sweden
I. I.	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
II. I.	1	1	1	1	2	2	2	2	2	2	2	2	2	2	2	3	3
III. I.	2	2	2	2	2	2	2	2	2	2	2	2	2	2	3?	3	3
IV. I.	2	2	2	2	2	2	2	2	3	3	3	3	3	3	3	3	3
V. I.	2	2	2	3	3	3	3	3	3	3	3	3	3	3	3	3	3
VI. I.	?	2	2	3	3	3	?	3	3	?	?	3	3	3	3	4	4
VII. I.	?	2	?	?	?	?	?	?	?	?	?	?	3	?	?	?	?
VIII. I.	?	?	?	?	?	?	?	?	?	?	?	?	?	?	?	?	?

The species-group of *Encrinurus sexcostatus* would appear to show a similar evolutional stage at similar horizons, though in this the line at present is incomplete.

Facts such as I have enumerated in the different groups of fossils with which we have mainly to deal in Lower Palæozoic rocks show that, if viewed from the evolutional standpoint, even the most meagre fauna may yet in many cases be made to yield a considerable amount of information as to the age of the beds containing it, for the evolutional succession, once established, can be applied anywhere, and the explanation of any apparent anomalies will be more correctly sought in the mutual relations of the rocks than in the faunas they contain.

Moreover, as regards the varying shallow-water faunas, even those which have a generally similar aspect may be shown definitely to be of different ages when one as a whole contains fossils at a different stage of evolution from the other, and the apparent similarity, so striking upon superficial examination, will then be regarded as determined by physical conditions and not by contemporaneity. Working on lines such as these we shall be enabled to visualise more definitely the conditions which governed the distribution of the different faunas in the remote past, and thereby acquire a more accurate conception of the changes in physical geography that must have taken place with the progress of time.

SECTION D.—ZOOLOGY.

MODERN ZOOLOGY:

SOME OF ITS DEVELOPMENTS AND ITS BEARINGS ON HUMAN WELFARE.

ADDRESS BY

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PRESIDENT OF THE SECTION.

Zoology has far outgrown its early boundaries when it could be defined simply as a part of natural history, and at no period has its growth been more rapid or more productive in results of scientific and practical importance than in the interval since our last meeting in this city. It is however impossible, even if time permitted, for any one observer to survey the many lines of activity in zoology or to record its contributions to knowledge in this fruitful period. I have thought it might be profitable to endeavour to take in retrospective glance the broad outlines of development of zoology during the last two or three decades, and then to limit our further consideration more especially to some of the relations of zoology to human welfare. The period under review has witnessed a growth of our knowledge of the living organism of the same order of importance as the progress in our knowledge of the atom. Never have investigators probed so deeply or with so much insight into the fundamental problems of the living animal; the means for observation and recording have become more delicate, and technique of all kinds more perfect, so that we can perceive details of structure and follow manifestations of activity of the organism which escaped our predecessors.

At the time of the last Liverpool Meeting and for some few years previously, a distrust of the morphological method as applied to the study of evolution had been expressed by a number of zoologists. At that meeting Professor MacBride put forward an able defence of morphology while recognising that the morphological method had its limitations, which must be observed if the conclusions are to rest on safe ground. Through undue zeal of some of its devotees morphology had been pushed too far on arid and unproductive lines, and rash speculation based on unsound morphology brought discredit on this branch of our science. It is now fully recognised that the observed resemblances between animals are due, some of them to genetic relationships, and others to convergent evolution, and therefore that the conclusions drawn from the study of morphology are to be interpreted with the greatest circumspection. There are some groups of animals, *e.g.* the earthworms, in regard to the evolutionary history of which we can never hope to receive help from palæontology; we must perforce make the best use we can of the morphological method applied, be it understood, with wide knowledge and deep insight. That careful systematic work, coupled with the skilful application of sound morphological

principles, is capable of yielding results of specific and general importance is well illustrated by the researches of Michaelsen and of Stephenson on Indian Oligochætes; these authors have been able to trace the lines of evolution of the members of the family Megascolecidæ so completely that we know their history as well as we know that of the Equidæ. Again, to take an example from a different category, the fine morphological work on the cell and on the nucleus and its chromosomes which we owe to Hertwig, Flemming, Boveri, van Beneden, Wilson and others, made possible the modern researches and conceptions in regard to inheritance and sex. The danger that morphology will be pushed to excess is long past; the peril seems to me to be rather in the opposite direction, *i.e.* that some of our students before passing on to research receive too little of that training and discipline in exact morphology by which alone they can be brought to appreciate how the components of the living organism are related to one another and to those of allied species or genera, and how they afford, with proper handling, many data for the evolutionist. I plead, therefore, for the retention of a sound and adequate basis of morphology in our zoological courses.

No one who engages in the study of morphological problems can proceed far without meeting questions which stimulate enquiry of a physiological nature, and, where means are available, resort to experimental procedure is the natural mode of arriving at the answer. That morphology is detrimental to or excludes experimental or physiological methods is entirely contrary to present day experience, and indeed the fruitfulness of the combination of morphology and physiology could have been amply illustrated any time during the last eighty years simply by reference to the work of Johannes Müller. The structure of an organism must be known before its co-ordinated movements can be adequately appreciated—morphology must be the forerunner of physiology.

Another of the basal supports of our science an appreciation of which, or better still a training in some branch of which, we must encourage is the systematic or taxonomic aspect. The student or graduate who is proceeding to specialise in experimental zoology or in genetics particularly requires a sound appreciation of the fact that the accurate determination of the genus and species under investigation is a primary requisite for all critical work—it is part of the fundamental data of the experiment and is essential, if for nothing else, to permit subsequent observers to repeat and perhaps to extend any given series of observations. Moreover, the systematic position of an animal is an expression of the final summary of its morphology and its genetic relationships, and it is from such summaries that we have to attempt in many cases—as, for example, in the Oligochætes already cited—to discover in a restricted group or order the probable course of evolution, though the method of evolution may not be ascertainable. From these summaries prepared by systematists issue problems for the experimental evolutionist and the geneticist. As Mr. Bateson has pointed out, it is from the systematist who has never lost the longing for the truth about evolution that the raw materials for genetical researches are to be drawn, and the separation of the laboratory men from the systematists imperils the work and the outlook of both.

Among the notable features of zoological activity during the last twenty-five years the amount of work on the physiology of organisms other than mammals must attract early notice in any general survey of the period. Eighty years ago Johannes Müller's physiological work was largely from the comparative standpoint, but for some years after his death the comparative method fell into disuse, and the science of physiology was concerned chiefly with the mode of action of the organs of man or of animals closely related to man, the results of which have been of outstanding importance from their bearing on medicine. Interest in the more general applications of physiology was revived by Claude Bernard ('Leçons sur les phénomènes de la vie,' 1878), and the appearance of Max Verworn's 'General Physiology,' in 1894, was in no inconsiderable measure responsible for the rapid extension of physiological methods of enquiry to the lower organisms—a development which has led to advances of fundamental importance. Many marine and fresh-water organisms lend themselves more readily than the higher vertebrates to experimentation on the effects of alterations in the surrounding medium, on changes in metabolic activity, on the problems of fertilisation and early development, on the chemistry of growth and decline, and to the direct observation of the functioning of the individual organs and of the effects thereon of different kinds of stimuli. The study of these phenomena has greatly modified our interpretation of the responses of animals and has given a new impetus to the investigation of the biology and habits of animals, *i.e.* animal behaviour. This line of work—represented in the past by notable contributions such as those by Darwin on earthworms, and by Lubbock on ants, bees and wasps—has assumed during the last two or three decades a more intensive form, and has afforded a more adequate idea of the living organism as a working entity, and revealed the delicacy of balance which exists between structure, activity and environment. This closer correlation of form, function and reaction is of the greatest value to the teacher of zoology, enabling him to emphasise in his teaching that for the adequate appreciation of animal structure a clear insight into the activities of the organism as a living thing is essential.

The penetrating light of modern investigation is being directed into the organism from its earliest stage. During the summer of 1897 Morgan discovered that the eggs of sea-urchins when placed in a two per cent. solution of sodium chloride in sea-water and then transferred to ordinary sea-water would undergo cleavage and give rise to larvæ, and J. Loeb's investigations in this field are familiar to all students of zoology. Artificial parthenogenesis is not restricted to the eggs of invertebrates, for Loeb and others have shown that the eggs of frogs may be made to develop by pricking them with a needle, and from such eggs frogs have been reared until they were fourteen months old. The application of the methods of microdissection to the eggs of sea-urchins is leading to a fuller knowledge of the constitution of the egg, of the method of penetration of the sperm, and of the nuclear and cytoplasmic phenomena accompanying maturation and fertilisation, and will no doubt be pursued with the object of arriving at a still closer analysis of the details of fertilisation.

The desire for more minute examination of developing embryos led to the more careful study of the egg-cleavage, so that in cases suitable for this method of investigation each blastomere and its products were followed throughout development, and thus the individual share of the blastomere in the cellular genesis of the various parts of the body was traced. This method had been introduced by Whitman in his thesis on Clepsine (1878), but it was not until after the classical papers of Boveri on Ascaris (1892) and E. B. Wilson on Nereis (1892) that it came into extensive use. About the time of our last meeting here, and for the next twelve or fifteen years, elaborate studies on cell-lineage formed a feature of zoological literature and afforded precise evidence on the mode of origin of the organs and tissues, especially of worms, molluscs and ascidians. A further result of the intensive study of egg-cleavage has been to bring into prominence the distinction between soma-cells and germ-cells, which in some animals is recognisable at a very early stage, *e.g.* in Miastor at the eight-cell stage. The evidence from this and other animals exhibiting early segregation of germ-cells supports the view that there is a germ-path and a continuity of germ-cells, but the advocates of this view are constrained to admit there are many cases in which up to the present an indication of the early differentiation of the germ-cells has not been forthcoming on investigation, and that the principle cannot be held to be generally established.

A cognate line of progress which, during the period under review, has issued from the intensive study of the egg and its development is experimental embryology—devoted to the experimental investigation of the physical and chemical conditions which underlie the transformation of the egg into embryo and adult. By altering first one and then another condition our knowledge of development has been greatly extended, by artificial separation of the blastomeres the power of adjustment and regulation during development has been investigated, and by further exploration of the nature of the egg the presence of substances foreshadowing the relative proportions and positions of future organs has been revealed in certain cases, the most striking of which is the egg of the Ascidian *Cynthia partita* (Conklin, 1905). Still further intensive study of the cytoplasm and nuclei of eggs and cleavage stages is required to throw light on the many problems which remain unsolved in this domain.

Progress in investigation of the egg has been paralleled by increase in our knowledge of the germ-cells, especially during their maturation into eggs and sperms, the utmost refinements of technique and observation having been brought to bear on these and on other cells. During the last thirty years, and especially during the latter half of this period, cytology has developed so rapidly that it has become one of the most important branches of modern biology. One of the landmarks in its progress was the appearance, at the end of 1896, of E. B. Wilson's book on 'The Cell,' and we look forward with great expectations to the new edition which, it is understood, is in an advanced stage of preparation. A great stimulus to cytological work resulted from the rediscovery in 1900 of the principle of heredity published by Mendel in 1865, which showed that a relatively simple conception was sufficient to explain the

method of inheritance in the examples chosen for his experiments, for in 1902 Sutton pointed out that an application of the facts then known as to the behaviour of the chromosomes would provide an explanation of the observed facts of Mendelian inheritance. In the same year McClung suggested that the accessory chromosome in the male germ-cells is a sex-determinant. These two papers may be taken as the starting-point of that vast series of researches which have gone far toward the elucidation of two of the great problems of biology—the structural basis of heredity and the nuclear mechanism correlated with sex. The evidence put forward by Morgan and his colleagues, resulting from their work on Drosophila, would seem to permit little possibility of doubt that factors or genes are carried in the chromosomes of the gametes, and that the behaviour of the chromosomes during maturation of the germ-cells and in fertilisation offers a valid explanation of the mode of inheritance of characters. The solution of this great riddle of biology has been arrived at through persistent observation and experiment and by critical analysis of the results from the point of view of the morphologist, the systematist, the cytologist, and the geneticist.

Among other important developments in the period, reference may be made to the great activity in investigation of the finer structure of the nerve-cell and its processes. By 1891 the general anatomical relations of nerve-cells and nerve-fibres had been cleared up largely through the brilliant work of Golgi and Cajal on the brain and spinal cord, and of von Lenhossék, Retzius, and others on the nervous system of annelids and other invertebrates. In these latter had been recognised the receptor cells, the motor or effector cells, and intermediary or internunciary cells interpolated between the receptors and effectors. In June 1891 Waldeyer put forward the neurone theory, the essence of which is that the nerve-cells are independent and that the processes of one cell, though coming into contiguous relation and interlacing with those of another cell, do not pass over into continuity. He founded his views partly upon evidence from embryological researches by His, but chiefly on results obtained from Golgi preparations and from anatomical investigations by Cajal. The neurone theory aroused sharp controversy, and this stimulus turned many acute observers—zoologists and histologists—to the intimate study of the nerve-cell. First among the able opponents of the theory was Apáthy, whose well-known paper, published in 1897, on the conducting element of the nervous system and its topographical relations to the cells, first made known to us the presence of the neurofibrillar network in the body of the nerve-cell and the neurofibrils in the cell-processes. Apáthy held that the neurofibrillar system formed a continuous network in the central nervous system, and he propounded a new theory of the constitution of the latter, and was supported in his opposition to the neurone theory by Bethe, Nissl, and others. The controversy swung to and fro for some years, but the neurone theory—with certain modifications—seems now to have established itself as a working doctrine. The theory first enunciated as the result of morphological studies receives support from the experimental proof of a slight arrest of the nerve-impulse at the synapse between two neurones, which causes a measurable delay in the transmission. The latest development

in morphological work on nerve-elements is the investigation of the neuromotor system in the Protozoa. Sharp (1914), Yocom (1918), and Taylor (1920), working in Kofoid's laboratory, have examined this mechanism in the ciliates Diplodinium and Euplotes and they describe and figure a mass—the neuromotorium—from which fibrils pass to the motor organs, to the sensory lip, and, in Diplodinium, to a ring round the œsophagus. The function of the apparatus is apparently not supporting or contractile, but conducting. By the application of the finest methods of micro-dissection specimens of Euplotes have been operated upon while they were observed under an oil-immersion objective. Severance of the fibres destroyed co-ordination between the membranelles and the cirri, but other incisions of similar extent made without injuring the fibrillar apparatus did not impair co-ordination, and experiments on Paramæcium by Rees (1922) have yielded similar results. While the experimental evidence is as yet less conclusive than the morphological, it supports the latter in the view that the fibrils have a conducting, co-ordinating function. Progress in our knowledge of the nervous system is but one of many lines of advance in our understanding of the correlation and regulation of the component parts of the animal organism.

The ciliate protozoa have been the subject during the last twenty years of a series of investigations of great interest, conducted with the purpose of ascertaining whether decline and death depend on inherent factors or on external conditions. While these researches have been in progress we have come to realise more fully that ciliates are by no means simple cells, and that some of them are organisms of highly complex structure. Twenty years ago Calkins succeeded in maintaining a strain of Paramæcium for twenty-three months, during which there were 742 successive divisions or generations, but the strain, which had exhibited signs of depression at intervals of about three months, finally died out, apparently from exhaustion. From this work, and the previous work of Maupas and Hertwig, the opinion became general that ciliates are able to pass through only a limited number of divisions, after which the animals weaken, become abnormal and die, and it was believed that the only way by which death could be averted was by a process of mating or conjugation involving an interchange of nuclear material between the two conjugants and resulting in a complete re-organisation of the nuclear apparatus. Jennings has shown that conjugation is not necessarily beneficial, that the ex-conjugants vary greatly in vitality and reproductive power, and that in most cases the division rate is less than before conjugation. Woodruff has since May 1, 1907, kept under constant conditions in culture a race of Paramæcium. During the sixteen years there have been some ten thousand generations, and there seems no likelihood of or reason for the death of the race so long as proper conditions are maintained. The possibility of conjugation has been precluded by isolation of the products of division in the main line of the culture, and the conclusion is justifiable that conjugation is not necessary for the continued life of the organism. The criticism that Woodruff's stock might be a non-conjugating race was met by placing the Paramæcia, left over from the direct line of culture,

under other conditions when conjugation was found to occur. Later observations by Erdmann and Woodruff show that a reorganisation of the nuclear apparatus of Paramæcium takes place about every twenty-five to thirty days (forty to fifty generations). This process, termed endomixis (in contrast to amphimixis), seems to be a normal event in the several races of Paramæcium which Erdmann and Woodruff have examined, and it is proved to coincide with the low points or depressions in the rhythm exhibited by Paramæcium. The occurrence of endomixis raises the question, to which at present there is no answer, as to whether this process is necessary for the continued health of the nuclear apparatus and of the cytoplasm of Paramæcium.

Enriques (1916) maintained a ciliate—*Glaucoma pyriformis*—through 2,701 generations without conjugation, and almost certainly without endomixis. From a single 'wild' specimen he raised a large number and found that conjugating pairs were abundant, so that the objection could not be made that this was a non-conjugating race. Enriques then began his culture with one individual, and examined the descendants morning and evening, removing each time a specimen for the succeeding culture. The number of divisions per day varied from nine to thirteen, and as there was no break in the regularity and rapidity of division, and no sort of depression, Enriques concluded that neither endomixis nor conjugation could have occurred, for these processes take some time and would have considerably reduced the rate of division. These results, especially if they are confirmed by cytological study of preserved examples, show that for Glaucoma neither conjugation nor endomixis is necessary for continued healthy existence. Hartmann's observations (1917) on the flagellate *Eudorina elegans* extend the conclusion to another class of Protozoa. He followed this flagellate through 550 generations in two and a-half years. The mode of reproduction was purely asexual, and there was no depression and no nuclear re-organisation other than that following fission. The evidence seems sufficient to confirm the view that certain Protozoa, if kept under favourable conditions, can maintain their vigour and divide indefinitely, without either amphimixis or endomixis.

Child (1915) states as the result of his experiments that the rate of metabolism is highest in Paramæcium and other ciliates immediately after fission—'in other words, after fission the animals are physiologically younger than before fission.' This view, that rejuvenescence occurs with each fission, derives support from the observations of Enriques and Hartmann, for no other process was found to be taking place and yet the vigour of their organisms in culture was unimpaired. If, then, fission is sufficiently frequent—that is, if the conditions for growth remain favourable—the protoplasm maintains its vigour. If through changes in the external conditions the division rate falls, the rejuvenescence at each fission may not be sufficient to balance the deterioration taking place between the less frequent divisions. Under such conditions endomixis or conjugation may occur with beneficial results in some cases, but if these processes are precluded there is apparently nothing to arrest the progressive decline or 'ageing' observed by Maupas and others. But further investigations are required on the physiology and morphology of decline in the protozoan individual.

The culture of tissues outside the body is throwing new light on the conditions requisite for the multiplication and differentiation of cells. R. G. Harrison (1907) was the first to devise a successful method by which the growth of somatic cells in culture could be followed under the microscope, and he was able to demonstrate the outgrowth of nerve-fibres from the central nervous tissue of the frog. Burrows (1911), after modifying the technique, cultivated nervous tissue, heart-cells, and mesenchymatous tissue of the chick in blood-plasma and embryonic extract, and this method has become a well-established means of investigation of cell-growth, tissues from the dog, cat, rat, guinea-pig, and man having been successfully grown. One strain of connective tissue-cells (fibroblasts) from the chick has been maintained in culture in vigorous condition for more than ten years, that is for probably some years longer than would have been the normal length of life of the cells in the fowl. Heart-cells may be grown generation after generation—all traces of the original fragment of tissue having disappeared—the cells forming a thin, rapidly growing, pulsating sheet. Drew (1922) has recently used instead of coagulated plasma a fluid medium containing calcium salts in a colloidal condition, and has obtained successful growth of various tissues from the mouse. He finds that epithelial cells when growing alone remain undifferentiated, but on the addition of connective tissue differentiation soon sets in, squamous epithelium producing keratin, mammary epithelium giving rise to acinous branching structures, and when heart-cells grow in proximity to connective tissue they exhibit typical myofibrillæ, but if the heart-cells grow apart from the connective tissue they form spindle-shaped cells without myofibrillæ. This study of the conditions which determine the growth and differentiation of cells is only at the beginning, but it is evident that a new line of investigation of great promise has been opened up which should lead also to a knowledge of the factors which determine slowing down of the division-rate and the cessation of division, and finally the complete decline of the cell.

For many lines of work in modern zoology biochemical methods are obviously essential, and the applications of physics to biology are likewise highly important—*e.g.* in studies of the form and development of organisms and of skeletal structures. Without entering into the vexed question as to whether all responses to stimuli are capable of explanation in terms of chemistry and physics, it is very evident that modern developments have led to the increasing application of chemical and physical methods to biological investigation, and consequently to a closer union between biology, chemistry, and physics. It is clear also that the association of zoology with medicine is in more than one respect becoming progressively closer—comparative anatomy and embryology, cytology, neurology, genetics, entomology, and parasitology, all have their bearing on human welfare.

Some Bearings of Zoology on Human Welfare.

The bearings of zoology on human welfare—as illustrated by the relation of insects, protozoa and helminthes to the spread or causation of disease in man—have become increasingly evident in these later years

and are familiar to every student of zoology or of medicine. At the time of our last meeting in Liverpool, insects were suspected of acting as transmitters of certain pathogenic organisms to man, but these cases were few and in no single instance had the life-cycle of the organism been worked out and the mode of its transmission from insect to man ascertained. The late Sir Patrick Manson, working in Amoy, had shown (1878) that the larvæ of *Filaria bancrofti* undergo growth and metamorphosis in mosquitoes, but the mode of transference of the metamorphosed larvæ was not determined until 1900. Nearly two years after our last meeting here the part played by the mosquito as host and transmitter of the parasite of malaria was made known by Ross. In addition to these two cases at least eight important examples can now be cited of arthropods proved to act as carriers of pathogenic organisms to man—*e.g.* Stegomyia—yellow fever, Phlebotomus—sandfly fever, tsetse-flies—sleeping sickness, Conorhinus—South American trypanosomiasis (Chagas' Disease), Chrysops—*Filaria* (*Loa*) *loa*, the flea *Xenopsylla cheopis*—plague, the body-louse—trench fever, relapsing fever and typhus, and the tick Ornithodorus—African relapsing fever. In selecting examples for brief consideration I propose to deal very shortly with malaria, although it is the most important of the insect-carried diseases, because the essential relations between the Anopheles mosquito and the parasite are known to everyone here. There still remain lacunæ in our knowledge of the malarial organisms. Ross and Thomson (1910), working in this city, showed that asexual forms of the parasite tend to persist in small numbers between relapses, and suggested that infection is maintained by these asexual stages. Such explanation elucidates those cases in which relapses occur after short intervals, but the recurrence of the attacks of fever after long intervals can only be explained by assuming that the parasites lie dormant in the body—and we know neither in what part of the body nor in what stage or condition they persist. Nevertheless, the cardinal points about the organism are established, and preventive measures and methods of attack based on a knowledge of the habits and bionomics of Anopheles have been fruitful in beneficial results in many parts of the world.

If we desire an illustration of the vast difference to human well-being between knowing and not knowing how a disease-germ is transmitted to man, we may turn to the case of yellow fever. When this pestilence came from the unknown, and no one knew how to check it, its appearance in a community gave rise to extreme despair and in many cases was the signal for wholesale migration of those inhabitants who could leave the place. But with the discovery that Stegomyia was the transmitting agent all this was changed. The municipality or district took steps to organise its preventive defences against a now tangible enemy, and the successful issue of these efforts, with the consequent great saving of life and reduction of human suffering in the Southern United States, in Panama, in Havana and in other places, is common knowledge. It is a striking fact that during 1922 Central America, the West Indies, and all but one country of South America were free from yellow fever, which has ravaged these regions for nearly two centuries. The campaign against Stegomyia is resulting, as a recent Rockefeller report

points out, in yellow fever being restricted to rapidly diminishing, isolated areas, and this disease seems to be one which by persistent effort can be brought completely under control.

In 1895 Bruce went to Zululand to investigate the tsetse-fly disease which had made large tracts of Africa uninhabitable for stock, and near the end of the same year he issued his preliminary report in which he showed that the disease was not caused by some poison elaborated by the fly—as had been formerly believed—but was due to a minute flagellate organism, a trypanosome, conveyed from affected to healthy animals by a tsetse-fly (*Glossina morsitans*). In 1901 Forde noticed an active organism in the blood of an Englishman in Gambia suffering from irregularly intermittent fever, and Dutton (1902) recognised it as a trypanosome, which he named *Trypanosoma gambiense*. In 1902 Castellani found trypanosomes in the blood and cerebro-spinal fluid of natives with sleeping sickness in Uganda, and suggested that the trypanosome was the causal organism of the disease. The Sleeping Sickness Commission (Bruce and his colleagues) confirmed this view, and showed that a tsetse-fly, *Glossina palpalis*, was the transmitter. Since then much has been learnt regarding the multiplication of the trypansosome in the fly and its transference to man. For some years this was believed to take place by the direct method, but in 1908 Kleine demonstrated 'cyclical' transmission, and this was shown later to be the principal means of transference of *T. gambiense*. In 1910 Stephens and Fantham described from an Englishman, who had become infected in Rhodesia, a trypanosome which, from its morphological characters and greater virulence, they regarded as a new species, *T. rhodesiense*, and its 'cyclical' transmission by *Glossina morsitans* was proved by Kinghorn and Yorke. Recent reports by Duke and Swynnerton (1923) of investigations in Tanganyika Territory suggest that direct rather than cyclical transmission by a new species of Glossina is there mainly responsible for the spread of a trypanosome of the *rhodesiense* type. The impossibility of distinguishing by their morphology what are considered to be different species of trypanosomes, and the difficulty of attacking the fly, are handicaps to progress in the campaign against sleeping sickness, which presents some of the most subtle problems in present day entomology and protozoology. Here also we come upon perplexing conditions due apparently to the different virulence of separate strains of the same species of trypanosome and the varying tolerance of individual hosts—on which subjects much further work is required.

The relation of fleas to plague provides one of the best and most recent illustrations of the necessity for careful work on the systematics and on the structure and bionomics of insects concerned in carrying pathogenic organisms. Plague was introduced into Bombay in autumn 1896, and during the next two years extended over the greater part of Bombay Presidency and was carried to distant provinces. The Indian Government requested that a Commission should be sent out to investigate the conditions. This Commission, which visited India in 1898-99, came to the conclusion (1901) that rats spread plague and that infection of man took place through the skin, but—and this is amazing to us at

the present day—' that suctorial insects do not come under consideration in connection with the spread of plague.' Further observations, however, soon showed this conclusion to be erroneous. Liston found in Bombay in 1903 that the common rat-flea was *Pulex* (*Xenopsylla*) *cheopis*, that it was present in houses in which rats had died of plague and in which some of the residents had become infected, that the plague-bacillus could multiply in the stomach of this flea, and that the flea would —in the absence of its usual host—attack man. These observations pointed to the importance of this flea in the dissemination of plague, and the Second Plague Commission, which was appointed and began work in 1905, definitely proved that *Xenopsylla cheopis* is the transmitter of the plague-organism from rat to rat and from rat to man. The mechanism of transmission of the plague-bacillus was worked out by Bacot and Martin in 1913. They showed that in a proportion of these fleas fed on the blood of septicæmic mice the plague-bacilli multiply in the proventriculus—which is provided with chitinous processes that act as a valve to prevent regurgitation of the blood from the stomach—and a mass of bacilli is formed which blocks the proventriculus and may extend forward into the œsophagus. Fleas in this condition are not prevented from sucking blood because the pharynx is the suctorial organ, but their attempts to obtain blood result only in distending the œsophagus. The blood drawn into the œsophagus is repeatedly forced backwards into contact with the mass of plague-bacilli and on the sucking action ceasing some of this infected blood is expelled into the wound. The transmission of plague depends on the peculiar structure of the proventriculus of the flea and on the extent to which, in certain examples, the plague-bacilli multiply in the proventriculus. Such 'blocked' fleas being unable to take blood into the stomach are in a starved condition, and make repeated attempts to feed, and hence are particularly dangerous.

Until 1913 it was believed that all the fleas of the genus Xenopsylla found on rats in India belonged to one species—*cheopis*, but in that year L. F. Hirst reported that the rat-flea of Colombo was *X. astia*, which had been taken off rats in Rangoon, and described by N. C. Rothschild in 1911. Hirst ascertained that this flea did not readily bite man if the temperature were above 80° F. A collection of 788 fleas from Madras City proved to consist entirely of *X. astia*, and Hirst suggested that the explanation of the immunity of Madras and Colombo from plague was the relative inefficiency of *X. astia* as a transmitter. Cragg's examination (1921, 1923) of 23,657 fleas obtained from rats in all parts of India shows that they include three species of Xenopsylla—namely, *cheopis*, *astia*, and *brasiliensis*. This last species is common in the central and northern uplands of peninsular India, but its bionomics have not yet been investigated. *Cheopis* is the predominant species in the plague areas, while *astia* is the common flea in those areas which have remained free from plague or have suffered only lightly. In Madras City, for instance, during the twenty-one years, 1897-1917, plague has occurred in twenty of these years, but the average mortality was only .013 per thousand—that is, though the infection has been repeatedly introduced there, it failed each time to set up an epidemic.

The significance of an imported case of plague depends in large measure on the local species of Xenopsylla. Hirst has made numerous attempts during the plague season in Colombo to transmit plague by means of *X. astia* from rat to rat, but with negative results, and *X. astia* was never found to behave like a 'blocked' *cheopis*.

The distinction of *X. cheopis* from *X. astia* is not an entomological refinement with purely systematic significance, but corresponds with a different relation of the species to the epidemiology of plague, and hence becomes a factor of great practical importance. If through these researches it has become possible by examination of the rat-fleas of a locality to estimate accurately its liability to plague, anti-plague measures may henceforward be restricted to those areas in which plague is likely to occur, *i.e.* where *cheopis* is the predominant flea. Thus a great economy of effort and of expenditure and a higher degree of efficiency may be achieved; in fact, the problem of the prevention or reduction of plague may be brought from unwieldy to practicable proportions. When it is remembered that since we last met in Liverpool some ten and a quarter millions of people have died in India from plague we have a more than sufficient index of the importance of a precise knowledge of the systematics, structure, and bionomics of the insect-carrier of *Bacillus pestis*.

Another of the outstanding features of the period under review has been the extensive and intensive study of the Protozoa. The structure and the bionomics and life-history of these organisms have been investigated with the help of the finest developments of modern technique. It is fitting here to record our acknowledgment to two staining methods—Heidenhain's iron-hæmatoxylin and the Romanowsky stain (including Giemsa's and Leishman's modifications), which have added greatly to our technical resources.

There is time to refer only to certain of the Protozoa which directly affect man. Twenty years ago our knowledge of the few species of Protozoa recorded from the human alimentary canal was defective in two important respects—the systematic characters and the biology of the species—so there was much confusion. Subsequent investigations, and especially those of the last ten years (by Wenyon, Dobell, and others), have cleared up most of the doubtful points, but owing to the difficulties of size and the paucity of characters available it is by no means easy in practice to distinguish certain of the species. Of the seventeen species now known to occur in the intestine of man *Entamœba histolytica* has received particular attention. This organism lives as a tissue parasite in the wall of the large intestine, where, as a rule, the damage caused is counterbalanced by the host's regenerative processes. But when the destruction outstrips the regeneration intestinal disturbance results, leading to the condition known as amœbic dysentery. The specific characters and the processes of reproduction and encystment of *E. histolytica* are now well ascertained, and it is realised that in the majority of cases the host is healthy, acting as a 'carrier' dangerous to himself, for he may develop into a case of acute dysentery, and to the community—for he is passing in his

fæces the encysted stage which is capable of infecting other persons. Whether an infected person will suffer from dysentery or act as a healthy 'carrier' apparently depends upon his own susceptibility rather than on any difference in the virulence of different strains of the Entamœba.

In all work with human Entamœbæ there is need for critical determination of the species, for, in addition to *E. histolytica*, a closely similar species, *E. coli*, is a common inhabitant of the intestine. This, however, is a harmless commensal, feeding on bacteria and fragments derived from the host's food. The distinction between the two species rests chiefly upon the characters of the nuclei and of the mature cyst—quadrinucleate in *histolytica* and octonucleate in *coli*—and considerable care and technical skill are requisite in many cases before a diagnosis can be given. And yet this distinction is definitely necessary in practice, for indiscriminate treatment of persons with Entamœba is indefensible; treatment is only for those with *histolytica*; it is useless for those with *coli*, and subjects them needlessly to an unpleasant experience.

A notable result of recent work is the proof that the more common intestinal Protozoa, formerly believed to be restricted to warmer countries, occur indigenously in Britain. This was first established by a group of observers in this city, and has been confirmed and extended by subsequent workers. There is good reason for believing that in this country the incidence of infection with *E. histolytica* is about 7 to 10 per cent., and with *E. coli* about five times as great (Dobell).

The discovery (1903) of Leishmania, the organism of kala azar and of oriental sore, added another to the list of important human pathogenic Protozoa, but the mode of transmission of this flagellate has not yet been proved.

Of the problems presented by the parasitic worms the most momentous are those associated with Ancylostoma and its near relative Necator, which are prevalent in countries lying between 36° N. and 30° S.—a zone which contains more than half the population of the earth. Heavy infection with Ancylostoma or with Necator produces severe anæmia, and reduces the host's physical and mental efficiency to a serious degree. Until 1898 there was no suggestion that infection was acquired in any other way than by the mouth, but in that year Looss published his first communication on the entry of the larvæ of Ancylostoma through the skin, and in 1903 gave an account of further experiments which proved that dermal infection resulted in the presence of worms in the intestine. At the meeting of this Association in Cambridge in 1904 Looss demonstrated to a small company his microscopical preparations showing the path of migration of the larvæ. His investigations served to establish the importance of the skin as the chief portal of entry of Ancylostoma, and pointed the way to effective methods of prevention against infection.

Another notable advance in helminthology is the working out of the life-cycle of Schistosoma (Bilharzia)—a genus of trematode worms causing much suffering in Egypt and elsewhere in Africa, as well as

in Japan and other parts of the world. These worms when mature live in pairs, a male and female, in the veins of the lower part of the abdomen, especially in the wall of the bladder and of the rectum. The eggs, laid in large numbers by the female worm, provoke inflammatory changes, and cause rupture of the veins of the organs invaded. Until about ten years ago the life-history of Schistosoma had been traced only as far as the hatching of the ciliated larva or miracidium which takes place shortly after the egg reaches water, but it was then shown that this larva is not, as had been held by Looss, the stage which infects man. Miyairi and Suzuki (1913) found that the miracidium of *Schistosoma japonicum* entered a fresh-water snail which acted as the intermediate host, and Leiper and Atkinson (1915) confirmed and extended this observation, and showed that the miracidia develop into sporocysts in which cercariæ are formed. We owe chiefly to Leiper's work (1915-1916) our knowledge of the life-history and method of entry into man of the Egyptian species of Schistosoma. He demonstrated that two species of this parasite occur in Egypt, and established that the miracidia develop in different intermediate hosts: those of *S. mansoni* enter Planorbis, while those of *S. hæmatobium* penetrate into Bullinus—the molluscs being abundant in the irrigation canals. The sporocysts produce cercariæ, which escape from the snails and gather near the surface of the water, and experiments with young mice and rats showed that the cercariæ attach themselves to the skin, enter, and reach the portal system from which they travel to the veins of the lower part of the abdomen. Infection of man takes place chiefly through the skin when bathing or washing in water containing the cercariæ, though infection may also occur through drinking such water. And so, at last, these worms which have troubled Egypt for at least thirty centuries have become known in all their stages, and measures for preventing infection—which were of great use during the War—have been devised, and curative treatment introduced.

Other recent helminthological researches deserve consideration did time permit, for there has been much excellent work on the life-history of the liver-flukes and lung-flukes of man, and the life-cycle of the tape-worm, *Dibothriocephalus latus*, was worked out in 1916-17. Mention should also be made of Stewart's investigations (1916-19) on the life-history of the large round-worm *Ascaris lumbricoides*, during which he made the important discovery that the larvæ on hatching in the intestine penetrate into the wall and are carried in the blood to the liver, and thence through the heart to the lungs, where they escape from the blood-vessels, causing injury to the lungs. The larvæ, now about ten times their original size, migrate by way of the trachea and pharynx to the intestine, where they grow to maturity. During last year Dr. and Mrs. Connal have worked out the life-history of *Filaria* (*Loa*) *loa* in two species of the Tabanid fly, Chrysops, and investigations on other Filarias have thrown light on their structure, but there is still need for further researches on the conditions governing the remarkable periodicity exhibited by the larvæ of some species (*e.g. F. bancrofti*; in some parts of the world the larvæ of this species are, however, non-periodic). The period under review has obviously been one of great activity in

research on helminthes, and fertile in measures tending to reduce the risks of infection.

Insects, protozoa and helminthes not only inflict direct injury on man; they also diminish his material welfare by impairing the health or causing the death of his horses, cattle and sheep, by destroying food crops during growth and, in the case of insects, by devouring the harvested grain. The measure of control which man can gain over insects, ticks and endoparasitic organisms, will determine largely the extent to which he can use and develop the natural resources of the rich tropical and sub-tropical zone of the earth

Other applications of zoology to human well-being cannot be dealt with owing to lack of time, but mention should be made of two—the researches on sea-fisheries problems which have formed an important branch of the zoological work of this country for forty years, and the studies on genetics which made possible an explanation of the mode of inheritance of a particular blood-group, and of some of the defects (*e.g.* colour-blindness and hæmophilia) and malformations which appear in the human race.

Maintenance of Correlation between the Branches of Zoology.

The rapid expansion of zoology has brought in its train the difficulty of maintaining the connection between its different branches. There is not only the mental divergence of the different workers, due to the necessity for specialised reading, thinking, and technique, but also in some cases spatial separation, and this seems to me to be the factor of greater importance. When modern developments of the subject necessitate expansion of the staff and of the working facilities it has not infrequently happened that one of the newer branches of the subject has been placed in another building, and unless careful arrangements are devised the dissociation tends to become more marked, so that, to take Mr. Bateson's example, the geneticist becomes separated from his colleague whose interests are more largely in systematic zoology. to their mutual disadvantage.

The actively growing physiological branch of zoology will, it is to be hoped, remain an integral part of our subject; for while there are close and friendly relations between the Department of Zoology and the Department of Physiology, the latter is mainly concerned with the training of medical students, and the teaching and research are consequently, in most Universities, chiefly directed to the physiology of mammals and of the frog. The medical physiologist cannot be expected to prosecute researches on the invertebrates—these are as a rule too far removed from the matters with which he is especially concerned—and yet many of the invertebrates have been found to be especially favourable for the investigation of fundamental problems which the morphologist with physiological leanings and training seems most fitted to undertake. It is a good sign that more students of zoology are including a course of physiology in their curriculum for the science degree, thus preparing themselves for work in comparative morphology and comparative physiology.

The association of zoology with physiology, and with botany through common problems in genetics and in general physiology, is becoming more intimate. The association of zoology with medicine has become of such importance, especially in regard to its parasitological and its physiological aspects, that clearly collaboration with our medical colleagues in teaching and in research should be as close as possible.

Zoology in the Medical Curriculum.

Much has been written and said in recent years about the place of zoology in the medical curriculum, and the present seems a favourable opportunity to reconsider the position and to ascertain the general opinion of the body of zoologists on this important matter. There can, I think, be no doubt that the value of zoology taught in its modern significance is being increasingly appreciated by the majority of our medical colleagues. The minority consists of two categories—those who have not taken the trouble to inform themselves of the subjects nowadays brought to the notice of medical students in the course of zoology, and who apparently consider that this is the one subject in the curriculum in which there has been no evolution since they were themselves first-year students thirty or forty years ago, and those who feel that the increasing pressure in the curriculum calls for curtailment of the teaching in what they believe to be the less important subjects. The first of these categories need not detain us, for an opinion based on obsolete data is valueless. Those in the second category merit serious consideration, but I believe even many of these would change their views if they knew more fully what is being done in the modern course of zoology to give the medical student a broad, scientific outlook. Even if the course on zoology were cut out the time would not be wholly gained for other work, because many of the subjects now dealt with in the course would require consideration in the teaching of anatomy and physiology. The attention of the medical student is nowadays directed in his course of zoology not so much to the study of details of 'types' as to the principles which certain chosen animals serve to illustrate. A reasonable knowledge of structure is obviously requisite before the working together of the parts can be understood, and before general principles can be profitably discussed. The student at that early stage of his education must have concrete examples to enable him to grasp the functions of organs, development, ideas as to the relationships of animals, heredity, evolution, and so on, and his work in the laboratory should give him the opportunity of observing for himself the important structural points on which the principles are based. The practical work cannot be limited to what the student can do for himself, for at this stage of his training there are many things which he ought to see but which are beyond his technical powers to prepare for himself, so that a good series of demonstration objects is necessary, care being taken that the student not only sees the specimens but appreciates their significance. As the time given to zoology is limited, the examples for study and the principles to be illustrated are to be carefully chosen, for the course in zoology is not only a discipline

but should give basal knowledge of value in the subsequent years of study; and, moreover, if the student can see that his zoological work bears on his later studies he will take much more interest in it. It is important, therefore, that the points of contact of his present with his future work should be successively indicated.

The details of the course of zoology for the first-year medical student will vary in the hands of different teachers, and it is well that they should be to some extent elastic. In a minimum course will be included the consideration of two or three protozoa, a cœlenterate, an annelid, an arthropod—and especially the features in which it presents advance as compared with an annelid, an elasmobranch fish, and a frog, the primitive features of the fish being emphasised, and the chief systems of organs of both vertebrates compared with each other and with those of a mammal. The functions of the principal organs of all these examples will be dealt with so far as they can be understood from the account of structure—this latter being sufficient to illustrate the principles involved, care being taken not to over-elaborate structural details. Man's place in nature should be considered either in the course of zoology or in that of anatomy. Other opportunities occur during the course in anatomy, and still more in physiology, for reference to the conditions in lower animals, and if more use could be made of these opportunities the linkage between zoology and the second-year subjects would become much more perfect, and would help in doing away with the water-tight compartments into which the average student considers his early medical education to be divided.

The course in zoology should be planned so as to give the student a wide outlook on structure and function, adaptation and environment, some knowledge of the germ-cells and their maturation, of fertilisation, growth, regulation, regeneration, decline and death, and an introduction to evolution, heredity and genetics—in general, it should aim at affording a broad conception of the activities and modifications of the organism as a living thing, and should educate the student to manipulate, to observe and record, and to exercise his judgment in matters of inference and of theory.

While some reference may be made in the first-year course to insects and parasitic organisms to indicate the relationship between zoology and pathology and public health, it has seemed to me for some years that the real instruction in entomology and parasitology should be given in the later part of the third or early in the fourth year along with the course in bacteriology. The first-year student, although keenly interested in the direct applications of zoology to medicine, is not competent at that early stage of his career to obtain full advantage from studies on parasites. In most Universities a certain amount of time is already set aside in the third year for the study of protozoa, and of helminthes and their eggs, and I have suggested to some of my colleagues in Edinburgh that the teaching on these subjects in the first and in the third year should be brought together in the latter year and remodelled to form a short course of lectures, demonstrations, and practical work to cover the essentials required for general practice in this country. By this time the student is much better fitted to appreciate the bearings of

this work. I am also inclined to the opinion that a short course of six or eight lectures—on which attendance might be voluntary—on heredity and genetics would be of value in the fourth year to the good student who has a little time at his disposal.

I should be glad if my colleagues would give the Section the benefit of their views on the first-year course of zoology for medical students, and on the provision of a course on entomology and parasitology about the third year of medical study.

SECTION E.—GEOGRAPHY.

THE GEOGRAPHICAL POSITION OF THE BRITISH EMPIRE.

ADDRESS BY

VAUGHAN CORNISH, D.Sc.,

PRESIDENT OF THE SECTION.

Part I.—The Position which has been occupied.

The British Empire, although situated in every continent, with shores on all the oceans, is seen to have a definite geographical position when we consider the ports of call which unite its lands and the naval stations which guard the communications. During the growth of the Empire eastward and westward from Great Britain, numerous harbours were held at different times, those retained being a selection unrivalled by the ports of any other State in commercial and strategic position. Our many oceanic islands give us, moreover, an important advantage in the selection of maritime stations for aircraft.

The naval station of Burmuda, well withdrawn from aerial attack, has a central position in the great western embayment of North America intermediate between the ocean routes which connect Great Britain with Canada and the West Indies. No foreign ports flank the route between Canada and the west coast of Great Britain. At the western gateway of the South Atlantic we have excellent harbourage in the Falkland Isles. Malta, the capital of our Fleet in the Mediterranean, has a commanding position at the Straits which connect the eastern and western basins, and the naval station at Gibraltar helps to ensure the junction of the Home and Mediterranean Fleet and to protect the Cape route. Our status in the Sudan, the vulnerable frontier of Egypt, is still maintained, and the British army which is kept in Egypt as garrison of the Suez Canal ensures our use of this gateway as long as we can navigate the Mediterranean. If that navigation be interrupted we can still oppose the seizure of the Isthmus, for we are able to send reinforcements by way of the Red Sea. East of Egypt the British island of Perim stands in the Straits of Bab-el-Mandeb, and the garrisoned fuelling station of Aden provides the necessary port of call on the routes to Bombay and Colombo. Colombo, in the Crown Colony of Ceylon, is at the parting of the ways for Australia and the furthest parts of our Asiatic possessions ,and Singapore stands at the narrow gateway of the shortest route between India and the Far East.

The Cape route to India and Australasia is improved by British ports of call in Sierra Leone, St. Helena, and Mauritius, and is more effectively dominated from British South Africa than at first appears, for although there is open sea to the south there are no useful harbours in the Antarctic continent, and on the African coasts the harbours are under British control for a thousand miles from Cape Town.

Of the six great foreign Powers the French alone are posted on the flank of both routes between Great Britain and the Indian Ocean, and no Great Power has its home territory on that ocean, or railway connection thereto from its home territory.

Thus the principal lands of the British Empire—Canada, the British Isles, South Africa, India, and Australasia—have good communications with one another across the Atlantic and Indian Oceans both in peace and war.

The conditions of strategic communication across the North Pacific, on the contrary, are adverse to us, owing mainly to the circumstance that we opened up British Columbia across the prairies and by the coasting voyage. Had our colonising route been across the Pacific, the Hawaiian Islands, which were first brought into touch with the Western world by the ships of the Royal Navy, would have been a British settlement and one of our first-class naval stations. As things happened, however, these islands were first needed by the Americans, and now form the essential western outpost of the United States Navy. Between them and British Columbia the ocean is empty of islands, and Fanning Island, south-west of Hawaii, with the adjacent small coral islands in our possession, are no adequate substitute, even apart from overshadowing by a first-class naval station in the neighbourhood. Thus there is no good strategic communication between Australasia and Canada across the North Pacific. In this connection it must be remembered that cousinship does not relieve the American Government from the obligations which international law imposes upon neutrals. It was not until three years after the outbreak of the Great War that America could offer us any facilities in the harbour of Honolulu which were not equally open to Germans. It must also be noticed that we have no control of the Panama route between New Zealand and Great Britian.

Turning to the question of communication between British Columbia and India, it is important to realise that the Pacific coasts of North America and Asia are in a direct line with one another, forming part of a Great Circle, so that there is no short cut across the ocean, as the map misleadingly suggests. Thus the course between Vancouver and Hong Kong is not only very long, but also closely flanked by the home ports of Japan and many outlying Japanese islands, so that its security in time of war depends upon the attitude of the Japanese.

When, therefore, we differentiate the routes on which we have well-placed naval stations and recruiting bases from those dominated by the ports of some other Great Power, we see that the lands of the Empire are united by the Atlantic and Indian Oceans and strategically separated by the North Pacific. Thus the form in which the Mercator map is usually drawn by British cartographers with Canada in the upper left and Australasia in the lower right corner is a good representation of our

maritime Empire. It shows the lands as connected by the Atlantic and the Indian but not by the Pacific Ocean; Great Britain, the naval and military headquarters of the Empire, on the central meridian; and Port Said and Cape Town as connecting positions between the western and eastern parts of the Empire.

Upon this map a symmetrical distribution of our lands is revealed when a Great Circle is drawn connecting Halifax in Nova Scotia, the eastern terminal port of the Canadian Pacific Railway, with Fremantle, the western terminal port of the Australian railway system. This truly direct line, twisted on Mercator's map into the form of the letter S, extends just half-way round the meridians but is somewhat shorter than the semi-circumference of the globe, the difference of latitude between Halifax, N.S., and Fremantle being less than ninety degrees. The line passes through Lower Egypt close to the Suez Canal following the general direction of the Main Track of the Empire, which is the steaming route from Canada to Great Britain, and thence by the Suez Canal to India and Australia. At one end of the line lies the Canadian Dominion, and at the other Australasia, to the north the British Isles, and to the south the Union of South Africa, the chief homes of the British nation. Our coloured peoples are also distributed symmetrically about the line, India being on the east, the Crown Colonies and the Protectorates of Africa on the west, so that it is the axis of symmetry of the Empire. Not far from its middle point is the Isthmus of Suez, where our direct line of sea communication is crossed by the only continuous route for the international railways which will connect our Indian and African possessions, and adjacent to the Isthmus is the central station of our airways.

Such is the form and position of the British Empire, regarded as a maritime organisation, which in fact it is.

The Empire thus mapped has an Intermediate Position among the commercial, national, religious, and racial communities of the world such as is occupied by no other State. The ocean routes must always be the Link between the two great land areas of the world, and in the present state of land communication provide the connection between the numerous independent systems of continental railways. The chief of these systems is based on the ports of Continental Europe, of which the greatest communicate with the ocean, and therefore with other railway systems, by way of the English Channel. Thus the island of Great Britain is intermediate between the principal termini of the European railways and the other railway systems. Its harbourage is unequalled by that of any country of Continental Europe, and its supply of shipbuilding material and coal exceptionally good. Thus the physical characters of the island accord with its position on the commercial map, and the Metropolitan British in their Intermediate Position have become the chief common carriers of international commerce. Much of this profitable business used to be in the hands of smaller European States, whose commerce eventually suffered from their inability to defend themselves against more powerful neighbours. Our merchant shipping is protected by the Royal Navy, but owing to the recent development of fighting aircraft, ships of war can no longer protect the island itself,

and since the close of the recent war this citadel of the Empire, the home of two-thirds of the white population, has been more exposed to attack from the Continent than at any previous time during the last eight hundred years.

The Suez Canal, where we have the principal control, is the gateway between the railway termini of Europe, the greatest manufacturing centre of the world, and those of the monsoon region of Asia, the greatest centre of population. It is also on the shortest route between the railways of North America and India.

The commercial and strategic importance of Singapore as an Intermediate Position between India and the Far East is enhanced by the circumstance that railway communication between them is debarred by the greatest mountain system in the world.

Hong Kong, at the chief gateway of Southern China, is typical of British maritime stations both in its Intermediate Position and in the facilities provided for the ships of other nations, which swell the vast tonnage entered and cleared at the port.

How far-reaching is the effect of our Intermediate Position is revealed by the important but little recognised fact that it is the British naval stations which would, if available, provide America with the best line for reinforcement of the Philippines, the Achilles' heel of the Republic. The distance of Manila from the naval shipbuilding yards of the United States is almost exactly the same by Suez and Panama, but the Pacific connection has never been good owing to the great distance between stations, and is now worse than before the Great War on account of the island mandates acquired by the Japanese. The relation of Port Said and Singapore to America and the Philippines is only one of many cases in which our position is intermediate between the home and Colonial possessions of a white nation. Thus the important French possession of Indo-China has to be reached from France either by way of the Suez Canal where we maintain a garrison, or by rounding the Cape where we have a national recruiting base, as well as a station of the Royal Navy. The true significance of our Intermediate Position has, however, been generally missed owing to a one-sided interpretation of strategical geography. An intermediate station, particularly a naval station, has commonly been regarded as a blocking position, a Barrier where freedom of movement can be interfered with. The historical fact is, however, that the harbours of the British Empire have also been a Link between nations. In the Great War the British Empire was the Link of the Allied and Associated Powers, and its geographical position is unequalled for making a benevolent alliance effective or checkmating the action of an alliance formed with a sinister purpose.

The British Empire provides in Canada the one Link between the European and American divisions of the white race, for public opinion in the United States adheres to the view that the New World, in the sense of North and South America, should be shut off and sheltered from the evils of a bad Old Europe.

In Tropical Australia the British, in the exercise of their discretion, have set up a Barrier between the white and coloured races. Australia is a land almost empty of aboriginals, which has for the most part

a climate in which British children thrive and develop true to type. In the great basin of the Murray River and its confluents, not far from the huge superficial deposit of brown coal in South Victoria, is a combination of fertile soil, forcing sun, water for irrigation and cheap electric power transmitted from the coal-field. This favoured region, the 'Heart of Australia,' as it has been called, with a population of only three million, is equal in size to France, Italy, and Germany combined, which have a population of more than one hundred and thirty million. The problem of Australian settlement is, however, complicated by the circumstance that the northern coast-lands lie in the Tropics, and have a climate which makes field work very arduous to white men. It is, moreover, uncertain if British families would continue true to ancestral type in this climate. If, however, settlers from the neighbouring monsoon lands of Asia be admitted, whose descendants would rapidly increase, it would be impossible to maintain a colour line between Tropical and Temperate Australia, and the rough labour of the Commonwealth would in time be done by coloured people. The fact that this labour is cheap would result in the employment of a great number of coolies instead of the use of machinery, and Australia might become a land of coloured workmen and white overseers. Circumstances, therefore, forced the Australians to decide whether their tropical belt should be a Link or a Barrier between white and coloured labour. The decision to errect a Barrier was taken early, and has been consistently maintained. The strategic responsibility of the decision is seen to be very great when we look into the future and reflect on the facts of population.

Of the 1,650 million people in the world, the whites number about 500 and the coloured 1,150. The former are mainly grouped on the two sides of the North Atlantic Ocean; of the latter, the greater part, about 800 million, are in the monsoon region of Asia, which includes India, Indo-China, China proper, and Japan. The Australian British are far from the main body of the white race and from Great Britain, the chief recruiting base of their own nation. On the other hand, the distance by sea between Townsville, Queensland, and the Japanese coast is no longer than the course of the coasting steamers from Fremantle to Townsville; and the other lands of Monsoon Asia are even nearer than Japan.

Enough is known of the relation between geographical environment and national well-being to declare with confidence that the decision to erect a Barrier against coloured labour in Tropical Australia is best both for the white race in Australia and for the coloured people of the monsoon region of Asia. Not only is Government much more difficult with a two-colour population, but the admission of coolie labour would deteriorate the national character of the Australians, for history shows that the greatest nations are those which provide their own working class. Turning from the Occidental to the broader humanitarian view, it is only necessary to look ahead in order to see that the admission of Asiatic coolies to a British homeland is unkind to their descendants. Those that remain unmixed in race will have a stunted existence as a community cut off from full national life, whilst the case of mulatto descendants is almost worse, for the children are not brought up in the

family of the British parent, and yet are cut off from the full tradition of Asiatic civilisation. Far better, then, that the Asiatic coolie should remain where the family life of his descendants will be part and parcel of national life.

Neither should it be assumed that there is not room in Asia for a large additional population. The pressure of population in China is largely due to the undeveloped condition of mining, factories, and communications. The coal-fields are unsurpassed in the world, and iron ore is abundant; if they were worked, and factories were based upon them, the new occupations and improved market for agricultural produce would provide at home for many of those who now migrate oversea. The rise in standard of living which may be expected to follow industrial development would also reduce coolie competition in the white borderlands of the Pacific. The further development of manufacture in India would operate in the same direction. The growth of a manufacturing population in China and India would stimulate cultivation and stock-rearing in the sparsely inhabited region under Asiatic rule which runs diagonally across the meridians from the Persian Gulf to the Amur, and includes the eastern provinces of Persia at the one end and Mongolia and Manchuria at the other. This has for the most part a light rainfall, but comprises much fine prairie country and some good agricultural land, whilst in the more arid tracts there are many great rivers fed from snow-fields and glaciers which could be made to irrigate large areas where the sun is as strong as in Australia. Adjacent to the Indo-Chinese peninsula are the East Indies, whose climate is suited both to Indians and Chinese, with great tracts of undeveloped land whose productivity is attested by luxuriant forest. The sparsely peopled regions of Asia near to India, China, and Japan by land and sea, and for the most part connected with them by ties of civilisation, provide an area for the overflow from these countries which is more than twice as large as Tropical Australia and British Columbia, together with California, Washington, and Oregon, the American frontier provinces of English-speaking labour.

India includes one of the most important borderlands within the Orient, that of the Mohammedan and Hindu worlds. The Punjab, with its great rivers and plain, is in such striking contrast to the mountains and plateau of Iran that we are apt to lose sight of the fact that, climatically, it more resembles the highland on the west than the rainy valley of the Ganges on the east. It is an eastern borderland of Islam, a religious world which is mainly comprised in the belt of dry country which stretches diagonally from the Atlantic shore of Morocco to the Altai Mountains. Delhi, under the Great Moghul, was an advanced capital of the Mohammedan world just within the Ganges valley, which is the headquarters of Hinduism. In this sub-imperial capital the two antagonistic civilisations are now linked to the government of the United Kingdom, and the age-long wars between them have ceased.

Up to the time of British predominance, India was the terminal position of Continental conquerors unused to the sea, who did not develop the advantages of a salient maritime position. The ports of India lie conveniently for a long stretch of coast-land on the great gulf which forms the Indian Ocean, and now, owing to the facilities provided by British

shipping, much of this coast-land has easier communication with India than with its own continental interior. Several British possessions in the parts of Africa adjacent to the Indian Ocean are in the Intermediate Position between the principal homelands of the black peoples and the overflowing population of India, and nowhere has the responsibility of our Intermediate Position called for more careful examination of the rights and interests of competing coloured races. The decision with reference to Kenya which has just been given by the home Government recognises the main physical regions in the coloured world as political divisions of the Empire within which the established races have special rights.

The Union of South Africa is the racial home of white men and of the more numerous coloured people who are indigenous to the country. It is, therefore, largely a land of white overseers and coloured labour, but here, as in the other Britains beyond the seas, there is an opposition to the introduction of coloured blood into white families which is not met with where Latin races are similarly situated. The Dutch families are at one with those of British stock in the maintenance of this racial Barrier.

From the foregoing facts it is clear that the British people, Metropolitan and Colonial,[1] are in a greater degree than any other nation the doorkeepers of the world in respect of economic, strategic, and racial communications.

Part II.—The Consolidation of the Position.

The consolidation of the position which the British Nation has won turns upon the future of colonisation within the Empire. We must therefore compare the number of the Metropolitan and Colonial British with that of other peoples within and without the Empire, and take account of the relation between the present population of the world and the area of its empty lands. The British Empire comprises the fourth part of mankind, but the ratio of white to coloured people in the Empire is only about one to six. The former are mostly of British stock, and belong to the Christian world. The latter are of many stocks, differing physically from each other as much as from the white people, and belonging to diverse religions. Their population is steadily increasing under British rule, and some of them have recently made advances in political organisation and industrial efficiency. Consequently, if the Empire is to be guided by the British, the numbers of our race must also increase. There is, however, a school of thought which considers that if our ideals of ethics and efficiency are once accepted by the coloured peoples, the racial complexion of the Empire will be unimportant, as public affairs will be regulated by our principles. This point of view, which may be termed in a general sense the missionary standpoint, does not take account of the contingency that British ideals implanted in coloured stock may receive alien development in future

[1] The introduction of the term 'Dominion' served to suggest emancipation from the Colonial Office, but the word Colonial as descriptive of a people has permanent historical value and therefore should not be allowed to lapse.

generations owing to biological causes. Our confidence in Western culture in general, and the British version of that culture in particular, is based more upon the power of adaptation which it has shown in our hands since the Renaissance and the Era of Oceanic discovery than upon any system of which we can hand over a written prescription. It is only in our own national communities, mainly composed of British stock, with minorities nearly akin, that we can be confident that British ideals will develop typically in the way of natural evolution. Therefore in our own interests and in that of the coloured races (who conflict among themselves) it is desirable to maintain the present proportion of the British stock, to whom the Empire owes the just administration of law and a progressive physical science.

The co-operation of the Union of South Africa in the Great War only became possible after the failure of an insurrection by part of the Boers. Since the number of persons of Dutch and British stock is about equal, an influx of British colonists is required in order to ensure unanimity between South Africa and the rest of the Empire.

Passing to the ratios between British population and foreign nations, we have to note that the population of Australia stands to that of Japan as about one to ten. The Japanese are a patriotic as well as an advanced nation, and claim equality with the white nations from patriotic motives. It is evident, therefore, that a strong reinforcement of British population is needed to maintain the doctrine of a white Australia. For the same reason New Zealand also needs reinforcement, since Australasia is strategically one.

The number and density of the population of Canada is exceeded in the proportion of about ten to one by the white population of the United States, hence it is inevitable that there should be a large flow of people from the latter country to the Dominion. As it is essential to unanimity in the Empire that the Canadians should continue to be British in sentiment and not become pan-American, a large immigration from Great Britain is required in Canada. Moreover, the population of Continental Europe outnumbers that of Great Britain[2] in the proportion of something like ten to one, and as emigrants go to Canada from many European countries there is a further call for British immigrants to maintain the British character of the Dominion.

We have next to note that the population of Great Britain, which is now forty-three million, outnumbers the combined population of Canada, Newfoundland. South Africa, Australia, and New Zealand in the proportion of two and a-half to one, and increases more rapidly than that of all these Dominions, more than three and a-half million being added in the decade 1901-11, in spite of an emigration which much exceeded the immigration. Thus the chief source available for the British peopling of the Dominions is the Metropolitan, not the Colonial, population.

In 1891 the late Mr. G. Ravenstein calculated from the rate of increase of population the time which remained before the unoccupied lands of the world would be settled and developed in accordance with

[2] In the present condition of home affairs in Ireland it seems best to leave its population out of the numerical reckoning for Imperial purposes.

their agricultural capabilities. This period he reckoned at about two centuries, by which time the population was calculated at 6,000,000,000 instead of the 1,600,000,000 which it had reached in 1891. The figure must not be taken to indicate the final population of the world, about which we know nothing, but the epoch marks finality of a certain kind—namely, the end of the colonising period of history as colonising has hitherto been conducted. The world will then be completely parcelled out among the nations, and since it is very difficult to displace a nation, it is probable that those which occupy the world at the end of the colonising period will remain in possession for a long time, even as time is reckoned in the pages of history. If we allow a generation for the set-back of the War we may roughly reckon our zero-time as 1923 instead of 1891, which, on the basis of Mr. Ravenstein's figures, would still give about two centuries, or six generations, in which to provide the temperate climates of the British Empire with a sufficiency of British stock to ensure the continuance of their British character.

There is, however, a school of thought which sees the salvation of the home country in a reduction of its population. I take their strategic argument first. It is contended that Great Britain would be safer in time of war if it had no more people than its farms can feed. Judging by France and the former Austro-Hungarian Monarchy, this would be about one-half of our present population, for our country is small though fertile. The conditions of our strategic security have, however, undergone a great change since 1914. The best plan of campaign for a combination of European Powers bent on overthrowing the citadel of the Empire would be an attack by combined air-fleets, which could be concentrated on London, the great manufacturing towns, and the ship-building yards, wholly destroying them one by one by intensive bombardment. This plan would be more effective than naval blockade, which it is very difficult to make complete, and is liable to bring in new belligerents owing to interference with neutral shipping. In order to have strategic security in this island we must therefore be able to meet the air-force of a European combination as well as carry out our traditional plan of despatching a powerful expeditionary force for the support of a friendly Power. This active defence requires large population and high devlopment of technical industries, and therefore could not be sustained by a rural Britain.

The economic argument for reduced population has received ready but uncritical assent owing to the great want of employment since the War. It is stated that this island will never be able to support in proper comfort a population of forty-three million, the present figure. But the population which can be sustained in a country depends jointly upon internal resources and geographical position. The commercial position of Great Britain is more favourable than that of any other island of equal size, and the large amount of good coal, besides iron ore and beds of salt, enable full advantage to be taken of the geographical position in manufacturing for export. According to the estimate made in 1905 the stock of accessible coal in the United Kingdom is sufficient to last more than four hundred years at the present rate of output, and an estimate made in 1915 gives a yet larger stock.

Moreover, no change in the distribution of available minerals can ever do away with the commercial advantage conferred by our central and focal position on the natural maritime routes. Hence the population which can be supported in Great Britain depends upon services to outside nations to a much greater extent than in most countries.

The population which can be maintained in our home country depends, therefore, to an exceptional degree upon the population and prosperity of the rest of the world, so that when the world again gets into its stride there should be improved conditions here, and as the population of the world grows so should the number of jobs in the country increase. There is, therefore, no sufficient ground for stating that we have passed or reached the limit of population which the island can ever support.

The teaching of those who advocate reduction of population as the salvation of Great Britain includes eugenic and ethical arguments. Thus it is said that very small families conduce to a high standard of civilisation since more care can be devoted to the child. This, however, leaves out of account the educative influence of the children of a family upon one another. Everyone knows that an only child is at a disadvantage in life. The world being of both sexes, and the society in which we move mainly of our own generation, the full home training for life is only obtained if each child have a brother and sister, which implies a family of at least four.

The desirability of birth-restriction among the poorer classes is strongly pressed on the plea that we are breeding to an increasing extent from inferior stock, and thereby lowering the national type. As far as the allegation relates to defectives, it is indisputable that most of them are among the poorest of the poor, and that their breeding is an injury to the community, as is also the admission of defective or criminal aliens, but these are categories quite apart from our great working-class community.

The professional families are far too few to maintain the supply of original genius needed for this country's advance, for genius is largely in the nature of a sport, and has to be replenished from a very large reservoir of population. To recruit the professions entirely from the present professional families would, therefore, in the long run be fatal to originality. On the other side of the picture, a working-class home is the best preparatory school for the colonial frontier, where to have few wants is better than the possession of many attainments.

We are told that an increase of population in Great Britain will pack the slums and thereby reduce us to the 'C 3' category of physique, but this argument takes too little account of the redistribution of urban population which has been going on for the last forty years. The density of population in central London has diminished, and factories have sprung up along the railways which radiate from the town. In 1911 the five Counties surrounding London, with their two included County Boroughs, contained no less than one million residents born in London who had migrated into these more rural districts. Migration, it should be observed, whether to or from the town, prevents the close breeding which used to be a serious disgenic factor in villages.

In 1911 the birth-rate in the towns of England and Wales was higher than in the rural areas, and the Registrar-General's Report states that even when these figures are corrected for the movement of the people the rural districts would only have increased at the same rate as the country at large, adding that 'these facts are worth noting in view of the assumption, sometimes loosely made, that the population of the towns would cease to increase if it were not recruited from the country.' In this connection it should also be noted that the proportion of London residents who are London-born has steadily increased from 1881 onwards.

The growth of our towns is no longer haphazard, but has entered on the stage of planning.

A great abatement of the contamination of town air by smoke has been shown to be practicable, and it is largely in the matter of smoke and crowding that towns have been hygienically inferior to the country, for country cottages are often as bad in themselves as slum houses, and their water supply much inferior. Moreover, the hygiene of towns has always been dependent on the circumstance that here the health of many people is affected by the carelessness of a few, and it follows that the hygienic conditions of urban life are capable of immense improvement when scientific knowledge becomes general. The experience of the War has shown that the popular notion of the inferior moral of townsmen was unduly pessimistic, for our urban regiments not only showed intelligence, but exhibited a sustained valour which has seldom been surpassed in the long annals of military history.

That emigration to the Dominions brings some economic benefit to the home country cannot be gainsaid, for trade returns show that an emigrant to the Dominions buys as much here as eleven emigrants to the United States, and therefore as much as many foreigners; but those who fear additions to our people also fear the moral effects of emigration. They say that emigration will take the best and leave the worst, and so produce a disgenic effect in the home country. But the individual emigration of to-day differs in this respect from the group migrations under political compulsion, or for conscience sake, which inflicted eugenic loss upon Spain, France, and England in bygone days. The best lad for the Dominions is not necessarily the best for the home country, and an Empire which comprises urban as well as rural States requires young men whose business tenacity is sufficient to resist the restlessness of youth not less than those who are instinct with the spirit of the frontiersman.

That a relative increase of female migration would benefit national character cannot be gainsaid, for at present the Dominion frontiers lack the due weight of feminine influence, whilst in Great Britain many women are denied the full development of their character which some natures only attain by wedlock and motherhood. The Census of 1911, unaffected by War losses, shows an excess of about 1,300,000 females in Great Britain and a deficiency of about 750,000 in the Dominions. The inequality of distribution as between Great Britain and the Dominions limits the possible marriage-rate, and therefore the total births, in a way to which no other nation is equally subject. If the

numbers in the Dominions be equalised as the result of special encouragement of female emigration, there will still remain a large excess of women in Great Britain who cannot be paired in the Empire unless the stream of emigrants who now leave the Empire can be for the most part deflected to the Dominions. In Great Britain the total number of families is limited by the number of males. In dealing with the size of family needed to maintain or increase population I do not reckon the present surplus of nearly two million women resulting from the joint effect of migration and war. At present our community appears to be in a transitional stage between the limitation of the family by chance and by choice, but the census shows, from the present age of marriage in Great Britain and the number of deaths before this age, that a general preference for the family of three children would not quite maintain the population, apart from migration. If, therefore, the size of family be universally decided by choice the number of the race cannot even be maintained, far less increased, under present conditions unless those who enter into matrimony cherish the ideal of a family of four children.

Unless the British race increase we cannot insure the internal peace and external security of the Empire, or the continuance of its beneficent work of enlarging commerce and restricting the range of war. Therefore the birth-rate in Great Britain should be maintained above the death-rate at least until the British population in the Dominions exceeds that in the Mother Country. The maintenance of the race will then rest chiefly with our people oversea, and, with their great resources, it should be possible for them to keep pace with the other growing nations.

SECTION F.—ECONOMIC SCIENCE AND STATISTICS.

POPULATION AND UNEMPLOYMENT.

ADDRESS BY

SIR WILLIAM H. BEVERIDGE, K.C.B.,

PRESIDENT OF THE SECTION.

The impression that the civilised world is already threatened with over-population is very common to-day. Many, perhaps most, educated people are troubled by fear that the limits of population, probably in Europe and certainly in this country, have been reached, and that a reduction in the rate of increase is an urgent necessity. Most, if they were asked to give reasons for their fear, would refer to one or both of two reasons: they would point to the enormous volume of unemployment in this country; they would say that economic science, at least at Cambridge, had already pronounced its verdict. I propose to begin by raising some doubts as to the validity of each of these arguments.

Unemployment No Proof of Over-Population.

The volume of unemployment in Britain is undoubtedly serious, and almost certainly unparalleled in past history. Those who see, as we now do, more than a million wage-earners whom our industry for years together is unable to absorb in productive employment may be excused if they draw the inference that there are too many wage-earners in the country. The inference, though natural, is unjustified. Unemployment in Britain can in any case prove nothing about the world as a whole. History shows that it does not prove over-population even in Britain.

During the last half of the nineteenth century, the industry of the United Kingdom was finding room for a rapidly increasing number of wage-earners with an admittedly rising standard of production and comfort. Through the whole of that period there was unemployment in the country. The percentage of trade unionists out of work never fell to zero; in no year since 1874 was it less than two; at more than one crisis it reached a height comparable if not equal to that which we have just experienced. During 1922 this percentage has averaged fifteen; but it averaged over eleven in 1879 and over ten in 1886. These figures are not on an identical basis and are therefore not absolutely comparable. Taken for one year only, they understate the relatively greater seriousness of our recent experience, since the unemployment percentage was high through a large part of 1921 as well as in 1922, and still continues high. But the difference is one of degree rather than of kind. The peril of inferring over-population from unemployment is conclusively shown.

The experience of 1879 was up to then unparalleled; probably it was as much worse than anything previously recorded as the experience

of 1922 appears worse than that of 1879. The experience of 1879, however, the record year of unemployment, heralded, not over-population and the downfall of British industry, but a period of expansion and prosperity which itself reached, if it did not pass, all previous records. 'Real wages,' which had risen thirty per cent. in the twenty years to 1880, rose even more rapidly in the next twenty years to 1900. Anyone who in 1879, looking at the half or three-quarter million unemployed, had argued that the existing population of the United Kingdom (then about thirty-four millions) was all that the country could support without lowering its standards, would have been lamentably discredited at once. Ten years later he would have found a population nearly three millions more, enjoying a real income per head that was a fifth greater, with the unemployment percentage reduced to two. Ten years later still the population had grown further in size and in prosperity; those trades had grown most rapidly in which there had been and continued to be the largest percentages of unemployment.

The problems of unemployment and of over-population are distinct; they are two problems, not one. Severe unemployment has occurred in the past without over-population, as a function of the organisation and methods of industry, not of its size. On the other hand, it is very doubtful if excessive growth of population has ever shown itself or would naturally show itself by causing unemployment. A more probable effect would be pressure to work more than before in order to obtain the same comforts; a fall of real wages per hour, by increase either of hours or of prices.

The same dependence of unemployment on the organisation and methods of industry, rather than on its size, appears if we look, not backwards in time, but round us in space. It has been pointed out by Professor Cannan that one of the few groups of economists who from our post-war sufferings can at least obtain the high intellectual satisfaction of saying 'I told you so,' is that which maintains that changes in the purchasing power of money are the most potent causes of the fluctuations in prosperity known as cycles of trade or booms and depressions. 'In the pre-war period booms and depressions swept over the whole western world at once and left their causes obscure. In 1922 we have been treated to a sharp contrast between two groups of countries, one group having boom and full employment, the other depression and unemployment, the difference being quite clearly due to the first group having continued the process of currency inflation, the other group having dropped it.' To bring this generalisation down to particular instances, we see in Central Europe a nation which assuredly should be suffering from over-population if any nation is; Germany, defeated in war, has been compressed within narrower limits, has lost its shipping and foreign investments, its outlets for emigration and trade, and now by high birth-rates is repairing with exceptional speed the human losses of the war. Germany may or may not be suffering from over-population. She certainly has not suffered from unemployment; she has had a boom stimulated by inflation of the currency. We see on the other hand Britain, victorious in war, with

expanded territories and the world open to her, pursuing a different, no doubt a better, currency policy and experiencing unexampled unemployment. To argue uncritically from unemployment to over-population is to ignore the elements of both problems [1]

Europe before the War.

Let us turn to the second argument, the argument from authority and, above all, from the authority of Mr. J. M. Keynes. No economic writing in our generation has obtained so wide a fame as that of Mr. Keynes on the 'Economic Consequences of Peace.' None, on its merits, has deserved more. With its main argument neither I nor, I think, any later impartial student will wish to quarrel. There are, however, in that book certain phrases about population, used incidentally, almost casually, which have none of the weight of the main argument. To these almost more than to anything else is to be attributed the general dread of over-population to-day; these call for examination.

In the second chapter of his book, Mr. Keynes paints a picture of Europe as an economic Eldorado, now devastated beyond repair by war and the peace, but even before the war threatened by internal factors of instability—'the instability of an excessive population dependent for its livelihood on a complicated and artificial organisation, the psychological instability of the labouring and capitalist classes and the instability of Europe's claim, coupled with the completeness of her dependence on the food supplies of the New World.' In naming the first of these factors of instability Mr. Keynes already passes the judgment that Europe's population was 'excessive.' Elsewhere in the same chapter he is more specific: 'Up to about 1900 a unit of labour applied to industry yielded year by year a purchasing power over an increasing quantity of food. It is possible that about the year 1900 this process began to be reversed, and a diminishing yield of Nature to man's effort was beginning to reassert itself. But the tendency of cereals to rise in real cost was balanced by other improvements.' A few pages further on he passes from possibilities to positive assertion; in the last years before the War 'the tendency towards stringency was showing itself . . . in a steady increase of real cost . . . the law of diminishing returns was at last reasserting itself, and was making it necessary year by year for Europe to offer a greater quantity of other commodities to obtain the same amount of bread.' In the seventh chapter is a wider and yet more explicit assertion of 'the increase in the real cost of food and the diminishing response of Nature to any further increase in the population of the world.' And so to Malthus. 'Before the eighteenth century mankind entertained no false hopes. To lay the illusions which grew popular at that age's latter end, Malthus disclosed a Devil. For half a century all serious economical writings

[1] In the United States, which no one suspects of over-population, 'there seems good ground for believing that in actual diminution of employment, the depression of 1921 was almost twice as acute as that of 1908' (Berridge: *Cycles of Unemployment*, p. 52). 1908 was one of the worst depressions hitherto experienced in America.

TABLE I.

AGRICULTURAL AND OTHER PRODUCTION AT CERTAIN EPOCHS.

	EUROPE.					COUNTRIES SETTLED FROM EUROPE.					EUROPE AND ITS SETTLEMENTS.		
	1880	1890	1900	1910	1920	1880	1890	1900	1910	1920	1890	1900	1910
Population (thousands)	311,619	338,847	373,517	422,853	423,000	—	75,596	91,451	111,829	131,432	414,443	464,968	534,682
Total Production (1000 quarters).													
Wheat	136,067	152,006	192,869	225,356	—	—	75,033	103,295	138,982	178,049	227,039	296,164	364,338
Rye	130,741	145,759	173,185	194,195	—	—	3,333	3,384	4,476	8,818	149,092	176,569	198,671
Barley	70,254	78,343	89,427	111,665	—	—	11,113	15,412	27,601	26,401	89,456	104,839	139,266
Maize	40,542	48,683	53,797	65,435	—	—	234,465	270,889	360,996	396,868	283,148	324,686	426,431
Four Crops	377,604	424,791	509,278	596,651	—	—	323,944	392,980	532,055	609,776	748,735	902,258	1,128,706
Area under Crop (1000 acres)													
Wheat	89,891	95,165	109,394	125,448	—	—	49,977	65,500	80,717	107,142	145,142	174,894	206,165
Rye	100,301	99,122	101,508	102,508	—	—	2,201	1,802	2,291	5,565	101,323	103,310	104,789
Barley	34,953	38,449	41,163	49,458	—	—	4,104	4,957	9,280	10,769	42,553	46,120	58,738
Maize	19,612	22,372	24,435	26,026	—	—	77,662	91,584	116,685	111,878	100,034	116,019	142,711
Four Crops	244,757	255,108	276,500	303,440	—	—	133,944	163,843	208,973	235,354	389,052	440,343	512,413
Yield per Acre (bushels)													
Wheat	12·11	12·78	14·10	14·37	—	—	12·00	12·62	13·78	13·30	12·51	13·55	14·14
Rye	10·42	11·76	13·65	15·16	—	—	12·11	15·04	15·63	12·87	11·77	13·67	15·17
Barley	16·08	16·30	17·38	18·06	—	—	21·68	24·86	23·79	19·34	16·83	18·19	18·83
Maize	16·54	17·41	17·62	20·11	—	—	24·15	23·66	24·75	28·38	22·64	22·39	23·90
Four Crops	12·34	13·32	14·74	15·73	—	—	19·50	19·19	20·32	20·73	15·40	16·39	17·62
Yield per Head (bushels)													
Wheat	3·49	3·59	4·13	4·26	—	—	7·94	9·04	9·94	10·84	4·36	5·10	5·45
Rye	3·36	3·44	3·71	3·68	—	—	0·35	0·30	0·32	0·54	2·88	3·04	2·97
Barley	1·81	1·85	1·92	2·11	—	—	1·18	1·35	1·97	1·58	1·97	1·80	2·08
Maize	1·04	1·15	1·15	1·24	—	—	24·81	23·70	25·82	24·16	5·47	5·59	6·38
Four Crops	9·70	10·03	10·91	11·29	—	—	34·28	34·38	38·06	37·11	14·43	15·51	16·89
Production per Head (cwt.)													
Coal	15·08	18·24	21·74	25·33	—	—	39·61	54·96	81·01	—	22·14	28·27	36·60
Iron Ore	1·93	2·06	2·73	3·47	—	—	3·98	6·00	8·68	—	2·41	3·37	4·57
Crude Steel	—	·45	·93	1·45	—	—	1·13	2·46	4·26	—	·58	1·22	2·02

held that Devil in clear prospect. For the next half-century he was chained up and out of sight. Now perhaps we have loosened him again.'

These quotations set the problem. The question is not indeed whether Malthus' Devil is loose again. Mr. Keynes has seen to that; he stalks at large through our lecture-rooms and magazines and debating societies. The question is rather whether Mr. Keynes was right to loose this Devil now upon the public. Was there in Europe or in the world as a whole before the War clear evidence, first, of 'a diminishing yield of Nature to man's effort'; and, second, of a 'rising real cost' of corn?

The Course of Agricultural Production.

The answer to the first question is given by the table of 'Agricultural and other Production at certain Epochs' which is printed above.

Notes to Table I.

The figures of acreage and corn production at the successive epochs are averages for the six years 1878-1883, 1888-1893, 1898-1903, 1908-1913, and for the two years 1920-21, or for as many of those years as were available in each case.

The populations are those given in censuses or official estimates relating to dates within six months of January 1, 1881, 1891, 1901, 1911 and 1921, or are estimated for about those dates (being the centre of the six years taken for averaging) where no such census was available.

The figures for 'Europe' relate to Austria, Belgium, Bulgaria, Denmark, France, Germany, Holland, Hungary, Italy, Roumania, Russia (with Poland), Serbia, Spain, Sweden, and the United Kingdom, containing between them 94% of the total population of Europe in 1910. Norway, Finland, Portugal, Switzerland, Greece, Turkey, Bosnia and Herzegovina and a few minor states alone are excluded. The figures for 'Countries settled from Europe' relate to Canada, United States of America, Argentina, Uruguay, Australia, and New Zealand. At the epochs 1900 and 1910 actual returns are available for all those countries; at the earlier epochs the yields or acreages or both have had to be interpolated for a few countries (of which Spain and Roumania are the most important).

The yields, acreages, and populations for 1920-21 are based on the statistics given in the year book for 1921 of the International Agricultural Institute. The yields and acreages for earlier years are based on the statistics in the annual Agricultural Returns published by the English Board of Agriculture and Fisheries. The populations for these earlier years in Europe are based on the statistics compiled by the International Statistical Institute) *Etat de la Population*, published 1916).

Weights have been converted into quarters on the basis of 480 lbs. to the quarter of wheat, rye, and maize, and 448 lbs. to the quarter of barley.

The figures for coal, iron ore and steel production are five or three year averages centering on the years 1880, 1890, 1900, 1910. For Europe the production is actually that of Austria, Hungary, Belgium, France, Germany, Italy (not steel), Russia (not iron ore), Spain (not steel), Sweden, and United Kingdom. For European settlements the United States contribute all the steel and all but a little of the iron ore; for coal Canada, Australia and New Zealand are included. The production 'per head' is based on the same populations as those used for agriculture in Europe and its settlements respectively.

The population for Russia at January 1, 1911, is obtained (as 133,500,000) by interpolation from censuses and estimates for earlier years and from the official estimate of 130,820,000 at January 1, 1910, given both in the Agricultural Returns of the English Board of Agriculture from which the acreages and crops are taken, and in the *Annuaire Statistique* of 1916 (*Etat de la Population*). The 1921 year book of the International Agricultural Institute gives for January 1, 1911, an estimate of 138,274,500. This is inconsistent both with the estimate for January 1, 1910, and with the census of 1897, requiring an impossible rate of increase. It must refer to an area larger than that covered by the crop returns.

The first section of this table shows at four successive epochs—1880, 1890, 1900, 1910—the total yield and acreage of corn and the yield per acre and per head of population in Europe as a whole (including Britain), with corresponding figures for coal, iron ore, and steel. The second section gives corresponding facts for the principal countries settled from Europe—Australia, New Zealand, the United States, Canada, and parts of South America. The third section covers Europe and its settlements together, practically the whole of the 'white man's countries.' The figures for each epoch represent an average of years, generally six, centering about the end of the year named. The records are not absolutely complete; one or two small European countries have been left out altogether; one or two gaps at the earlier epochs have to be filled by estimate or interpolation. The substantial accuracy of the main results is beyond question.

The European section shows at each successive epoch a greatly increased population and acreage under corn, and a production increasing faster than either, so that yield per head and yield per acre alike both rise materially and steadily. Nature's response to human effort in agriculture, on each unit of soil and for each unit of total population in Europe, has increased, not diminished, up to the very eve of the War. Needless to say, this greater production of corn has not been due to a shifting of population from industry to agriculture, and has not been offset by a decline of manufacturing. The general movement of population has probably been in the opposite direction, from agriculture to industry; the output of coal, iron ore, and steel, the basic materials and products of industry, has risen yet more rapidly than the output of corn.

There is no trace of reaction, either in industry or in agriculture, in the last ten years of the table; nothing to suggest a turning-point at 1900. It is true that the rate of increase in the yield of corn per head and per acre from 1900 to 1910 is less than in the preceding decade, but it is as great as in the decade from 1880 to 1890. In any case, a slowing down in the rate of increase proves nothing. Corn is produced only to be consumed, and there is a limit to consumption. In the best and most progressive of all possible worlds, the consumption, and so the production, per head of wheat, rye, barley, and maize could not rise endlessly; when saturation-point had been reached the yield per head of these elementary necessaries would cease to rise, and the people would use their increasing powers over Nature to win luxuries and leisure. Something of this movement is already seen in the growth of wheat at the expense of rye between 1900 and 1910.

The second section of the table, covering the countries settled from Europe, begins only in 1890, but can be continued to 1920. It shows a very similar picture, not a markedly better one, in agriculture up to the War. From 1890 to 1910 the yield per acre of wheat has increased in the settlements a little faster than in Europe (15 against 12½ per cent.), but that of all crops taken together has increased more slowly (4 against 18 per cent.). The yield per head has also increased for wheat a little faster in the settlements than in Europe (25 against 19 per cent.), and for all crops a little more slowly (11 against 12½ per

cent.). The actual yield per head is, of course, much higher in the settlements; the yield per acre is lower for wheat, though higher for the other crops.

In general, as we find European agriculture more progressive than might have been expected, so we find the superiority of the new lands in that field less clear. It is in the industrial field, with doubled or trebled output of coal, iron ore, and steel per head between 1890 and 1910, that the progress of Europe's settlements is most marked.

In the third section of the table, taking Europe and its settlements together, we see progress, both in yield per acre and in yield per head of the four crops, more marked from 1900 to 1910 than from 1890 to 1900, and nothing to suggest a limit to the expansion of the white races in the countries which they hold.

The inclusion of Russia in any statistical table induces an element of uncertainty; it is difficult to be sure that figures for successive years relate to the same area. As a check upon this a second table has been prepared, giving figures for Western and Central Europe; that is, Europe without Russia and Poland. The broad results of this table from 1880 to 1910 are the same as those for Europe as a whole. The yield per acre for each crop and for all crops together is at each epoch higher than when Russia is included and has increased more rapidly. The yield of all crops per head of population has also increased, though less rapidly than for Europe as a whole; this is natural, for the exclusion of Russia means the exclusion of a country which has suffered least from urbanisation.[2]

The main interest of the second table lies in the fact that it can be continued to a fifth epoch—1920—after the War. It shows that at that epoch the total production of wheat in Western and Central Europe was back again near the point where it stood in 1880; for the four crops together, production was about half-way between 1880 and 1890. In acreage under cultivation Europe had gone back still further, probably fifty years at least; the yield per acre was at the point where it stood twenty or thirty years before. The population of course was much greater. Taking the years 1920-1921 together, two and three years after the last shot of the Great War had been fired, Western and Central Europe in total agricultural production had gone back a generation; in production per head of population it had gone back fifty years and more. If Russia and Poland could be included the comparison

[2] The maintained increase in the yield per acre and per head of total population in Western and Central Europe is remarkable, in view of the common assumption that in 'old countries' the point of maximum return to agriculture has long been reached. Unfortunately actual census figures of occupations are available only for seven countries (Austria, Belgium, Denmark, France, Hungary, Italy, and the United Kingdom), omitting all-important Germany; these show for the seven countries a stationary yield of corn per head of the total population and a markedly higher yield per head of the agricultural population in 1910 than in 1900 or 1890. The figures themselves are open to criticism, but it seems safe to assume that in Western and Central Europe as a whole, with the great industrial states of Germany and Britain, the agriculturists form, from 1880 onwards, a diminishing proportion of the total population; per head of those actually employed on the land the yield must have risen yet more markedly than appears in the tables.

TABLE II.

AGRICULTURAL PRODUCTION IN WESTERN AND CENTRAL EUROPE.

Epoch	1880	1890	1900	1910	1920
Population (thousands)	225,613	242,847	264,517	289,893	291,713
Total Production (1000 quarters)					
Wheat	110,796	120,311	137,635	149,466	112,924
Rye	57,196	62,904	76,146	91,949	55,738
Barley	53,580	55,575	60,163	63,738	51,602
Maize	38,205	45,725	48,516	57,763	51,712
Four Crops	259,777	284,515	322,460	362,916	271,976
Area under Crops (1000 acres)					
Wheat	59,960	61,448	63,287	65,139	57,456
Rye	30,716	31,477	31,128	32,305	23,521
Barley	21,752	21,873	20,740	21,718	20,746
Maize	17,849	20,142	21,455	22,147	22,534
Four Crops	130,277	134,940	136,610	141,309	124,257
Yield per Acre (bushels)					
Wheat	14·78	15·66	17·40	18·97	15·72
Rye	14·89	16·00	19·54	22·77	19·40
Barley	19·71	20·33	23·21	23·48	19·90
Maize	17·12	18·16	18·09	20·86	18·36
Four Crops	15·95	16·87	18·88	20·55	17·48
Yield per head (bushels)					
Wheat	3·93	3·97	4·16	4·13	3·10
Rye	2·03	2·07	2·30	2·54	1·53
Barley	1·90	1·83	1·82	1·76	1·41
Maize	1·35	1·51	1·47	1·59	1·42
Four Crops	9·21	9·38	9·75	10·02	7·46

Note to Table II.

The countries included up to 1910 are those forming ' Europe ' in Table I, with the exception of Russia and Poland.

For 1920 the area is nearly but not quite the same. The Polish war gains from Germany and Austria, being reckoned with Poland in the latter year, are excluded. On the other hand, Bosnia, Herzegovina and Montenegro (now part of the Serbo-Croat-Slovene state), Bessarabia (gained by Roumania from Russia), and the Serbian and Bulgarian gains since 1910 from Turkey are included. So far as can be judged, the excluded regions are somewhat less in area (122,000 square km. against 165,000) and somewhat greater in population (11,000,000 against 6,000,000 in 1911) than those included ; that is to say, the term ' Western and Central Europe ' in my table represents a slightly larger area and a slightly smaller population in 1920 than in 1910. The differences, however, are unimportant ; substantially the exclusions and inclusions balance one another and the total regions remain comparable.

would be worse. To point the contrast, we have the figures for Europe's settlements; from 1910 to 1920 a further growth of acreage under crops and of crops per acre, and a yield per head of population only slightly less.

This result is only incidental to the present inquiry. The main object of my calculations has been to test whether the facts suggested any diminution of returns to agriculture in Europe between 1900 and 1910. Having regard to Mr. Keynes' words, I expected to find in the last years before the war a falling yield in Europe, balanced by increased drawing on the virgin lands of the new world. Actually we find in Europe, decade by decade to the eve of war—population rising, acreage under corn rising, total production rising still more, so that we get a greater yield per acre and per head of the total population.[3]

The Movement of Corn Prices.

The answer to our second question, as to the real cost of corn, is as certain and hardly less surprising. If before the War it was becoming 'necessary year by year for Europe to offer a greater quantity of other commodities to obtain the same amount of bread,' the money price of corn must have been rising relatively to the money price of other commodities. There is no trace of such a rise; the movement was in the opposite direction; up to the eve of war the price of corn was falling relatively to the price of other commodities.

Table III shows the movement of wholesale prices from 1871 to 1913 as recorded in the two best-known British indices: that of the

[3] Detailed examination of the figures yields a number of interesting results which can only be briefly indicated here:

(1) The progress shown for all the countries taken together represents a general movement in the fifteen countries taken separately. Taking wheat alone, from 1880 to 1910 every country for which figures are available shows a large increase in the yield per acre, varying from 18 per cent. in France to 68 per cent. in Germany, and averaging 43 per cent.; the other countries show large increases from 1890 to 1910. Even from 1900 to 1910 of the fifteen countries every one but three shows an increased yield per acre; the United Kingdom is stationary and France has a trifling decline; the Danish figures are incomplete and abnormal. More surprising still, every one but four (Belgium, France, Holland and United Kingdom) shows an increase of wheat per head of total population in the decade. For crops other than wheat the figures are less uniformly progressive; generally between 1900 and 1910 yield per acre increased in each country for each crop, except barley (which increased in eight and decreased in six countries), but yield per head of total population increased only for wheat. This greater progress of wheat is in itself a sign of greater ease rather than stringency; it represents a rising, not a falling, standard of life.

(2) During the thirty years 1880 to 1910 the total acreage under each crop and the yield per acre, in Europe as a whole, have both grown. But the rates of growth for acreage and for yield per acre vary inversely. The acreage has increased most for barley (41 per cent.); next for wheat (38 per cent.); next for maize (33 per cent.); and least of all for rye (2 per cent.). The yield per acre has risen most for rye (45 per cent.); next for maize (22 per cent.); next for wheat (19 per cent.); least for barley (13 per cent.). This is an interesting statistical confirmation of expectations based on economic theory. The greater total production has been secured in wheat and barley mainly by bringing fresh lands under cultivation; in maize and rye, mainly by getting more out of lands already cultivated.

TABLE III.

RELATIVE MOVEMENTS IN WHOLESALE PRICES.

Board of Trade Index.

	All Articles	Corn	Meat & Dairy Products	Coal & Metals	As percentages of all articles (Col. 1) Corn	Meat & Dairy Products	Coal & Metals
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1871–80 .	138	166	119	81	126	86	59
1881–90 .	111	129	108	60	116	97	54
1891–00 .	95	108	96	65	113	101	67
1901–10 .	101	106	104	77	106	103	76
1911–13 .	114	116	115	84	102	101	74

Sauerbeck Index.

	All Articles	Vegetable Food	Minerals	As percentages of all articles (Col. 1) Vegetable Food	Minerals
	(1)	(2)	(3)	(4)	(5)
1851–60	94	98	99	104	105
1861–70	100	95	90	95	90
1871–80	96	96	98	100	101
1881–90	75	71	73	95	98
1891–00	66	61	73	92	110
1901–10	73	65	89	89	122
1911–13	83	72	105	87	126
1919	206	179	220	87	107
1920	251	227	295	90	117
1921	155	143	181	92	117
1922	132	108	137	82	104

Board of Trade and that of Sauerbeck. Both indices refer formally to the United Kingdom only, but there can be little danger in taking them as an indication of world conditions; United Kingdom prices from 1871 to 1913 must have followed world prices in all important movements.

From the early 'seventies prices generally first fell heavily to about 1896 and then rose, though not to the height from which they had fallen; that is to say, the value of money in relation to commodities first rose and then fell. Through this complete reversal in the movement of prices generally, the price of corn in relation to other articles has moved steadily—and downwards. Decade by decade from 1871 and to the last three years before the War the price of corn, as recorded by the Board of Trade, has fallen relatively to prices as a whole (column 5); with less regularity, but even more markedly, the relative price of coal and metals has risen (column 7). The result of these two movements is startling; to get in 1911-13 the same amount of corn as in 1871-80 or 1881-90, it would have been necessary to offer, not more coal and

metals at the later than at the earlier dates, but one-third less. The Sauerbeck index leads to substantially the same results; it shows from 1871-80 onwards a steady fall in the price of vegetable food and an even greater rise in the price of minerals relatively to all articles (columns 4 and 5); the cost in terms of minerals of a given quantity of vegetable food would have been one quarter to a third less on the eve of the War than it had been a generation before. Both indices point emphatically to a falling, not a rising, real cost of corn.

Index numbers of wholesale prices are open to criticism, in this connection as in many others, because they refer mainly to raw products and give little or no representation to manufactured articles. It would be consistent with the figures quoted above to argue that though the price of coal and of other minerals, which are the basis of manufacturing, had risen relatively to corn, the price of manufactured articles themselves as a whole had fallen relatively to corn. Such a result, paradoxical as it is, could occur in two ways: either if increases in manufacturing efficiency reduced the cost of manufacture or distribution, or if a superfluity of labour fit only for industry, as distinct from agriculture, reduced the reward to such labour, by an amount sufficient in each case to outweigh the increased cost of coal and other minerals. The first is a real possibility; it is just in the spheres of manufacturing and distribution that increased efficiency most naturally accompanies a growth in population and that invention and organisation win their last victories over diminishing returns. But a cheapening of manufacture in this way involves not a decreasing but an increasing return to each unit of labour in industry; it would cause a fall of the real cost of corn measured in labour. The second way assumes a fall in real wages of industrial workers both absolutely and relatively to those of agriculturists such as quite certainly has not taken place in Europe.

In regard to Europe as a whole we find no ground for Malthusian pessimism, no shadow of over-population before the War. Still less do we find them if we widen our view to embrace the world of white men. Mr. Keynes' fears seem not merely unnecessary but baseless; his specific statements are inconsistent with facts. Europe on the eve of war was not threatened with a falling standard of life because Nature's response to further increase in population was diminishing. It was not diminishing; it was increasing. Europe on the eve of war was not threatened with hunger by a rising real cost of corn; the real cost of corn was not rising; it was falling.

Room for Expansion.

I have dealt at some length with Europe before the War because that is Mr. Keynes' theme; in his view the society that seems bent on self-destruction by the Carthaginian peace that crowned the War was already in deadly peril from Nature. If now, with better assurance as to the past, we look for a moment at the distant future of the European races, the first though not the only point for consideration is the extent of the world's untouched or half-used resources in land and minerals. On this point, unfortunately, the existing information goes only part of the way. It is certain that enormous areas of the

earth which are fit for cultivation are not yet cultivated at all, and that of other areas only the surface has been scratched; but it is not certain how great the areas that could be cultivated are; how much of the land that is now unproductive of anything must for ever remain so.

In most European countries from 70 to 95 per cent. or more of the total area is now classed as 'productive'; it is being turned to some use—as arable, pasture, forest, and the like. In nine provinces of Canada (excluding the desolate Yukon and North-West Territories) the percentage of all the land that now produces anything is 8, in Siberia 18, in Australia 6, in South Africa 3. Even for the United States it is only 46, and for European Russia 55.[4] Part, no doubt, of the 'unproductive area' in all those countries is beyond possibility of cultivation; it is impossible on the present information to say how large a part. But the figures as they stand are eloquent of how little the European races have yet done to fill the lands that they hold; how ample the room for their expansion. Any suggestion that these races have reached or are within sight of territorial limits to their growth hardly deserves serious consideration.

Material Progress in Britain.

It is reasonable to suppose, however, that Mr. Keynes, though he speaks throughout of Europe, though he emphasises his European standpoint, was at heart concerned mainly for his own country, and may thus have generalised impressions derived from Britain. For us at least the position in these islands, rather than that in Europe or in industries as coal or iron mining or pig-iron production. Britain is the last years before the War and ask if all was then well and the prospect cheerful, we get no clear answer to our question. The picture that our economic records paint is half in shadows; to many the shadows will seem ominous of ill.

Unfortunately on this issue, so vital to our interests, the use of statistical tests is peculiarly difficult. The yield of our soil in agriculture is clearly irrelevant; only less so is the yield in such elementary industries as coal or iron mining or pig-iron production. Britain is essentially a manufacturing, commercial, and financial country; the return to its labour is measured by its output or gain from finished articles and services which themselves, by their infinite variety, escape all measurement. Current statistics both of production and of prices refer mainly to raw materials or food; they miss the main features of British economic life and service.

With this warning I invite consideration of the accompanying table of 'Material Progress in the United Kingdom relative to Population.' The table shows at six successive epochs, beginning with 1860 and ending with 1910, the course of some of the most important indices of economic conditions. The figure for each epoch is an average for ten years in which the epoch is central; thus for '1860' the average of 1855-64 is taken, for '1870' the average of 1865-74, and so on; for the last epoch, '1910,' the average is for the nine years 1905-13 alone; all War years are omitted. The various indices cover the activity

[4] *International Yearbook of Agricultural Statistics*, 1921, pp. 20-21.

TABLE IV.

Material Progress in United Kingdom relative to Population.

Epoch	Coal Production per head. tons	Pig-iron Production per head. cwt.	Ship-building Tonnage per 1000.	Raw Cotton Consumption per head. lbs.	Raw Wool Consumption per head. lbs.	Exports Index*	Real Wages†	Real Income per head‡ at 1913 prices. £	Consumption of Food, Drink, etc.§	Housing‖ (Scotland)
1860 (1855–64) .	2·62	2·70	9·72¶	28·1	—	48·5	60·7‡‡	26·0‡‡	86·1	43·4
1870 (1865–74) .	3·59	3·60	13·52	33·5	10·40	71·6	67·8	29·6	92·8	46·1
1880 (1875–84) .	4·21	4·20	15·99	38·6	10·25	80·0	77·4	34·1	103·5	49·2
1890 (1885–94) .	4·62	4·00	16·68	40·6	11·85	97·4	91·6	40·0	109·8	51·8
1900 (1895–04) .	5·22	4·30	20·36	40·1	12·35	102·7	101·0	45·9	120·9	54·3
1910 (1905–13) .	5·89	4·34	21·49	42·2**	12·56††	126·1	101·0	46·9	122·9	56·4
1860 (1855–64) .	50·2	62·8	47·7	70·0	—	47·2	60·1	56·6	71·3	79·9
1870 (1865–74) .	68·8	83·7	66·4	83·5	84·2	69·7	67·1	63·8	76·8	84·7
1880 (1875–84) .	80·6	97·7	78·5	96·3	83·0	77·9	76·6	74·3	85·7	90·6
1890 (1885–94) .	88·5	93·0	81·9	101·2	96·0	94·9	90·7	87·1	90·9	95·4
1900 (1895–04) .	100·0	100·0	100·0	100·0	100·0	100·0	100·0	100·0	100·0	100·0
1910 (1905–13) .	112·8	100·9	105·5	105·2	101·7	122·8	100·0	102·2	101·7	103·9

Notes to Table IV.

* Value of exports per head divided by Sauerbeck Index of Wholesale Prices. The 'actual figures' are index-numbers on basis 1900=100.

† Mr. G. H. Wood's Index Number (*Statistical Journal*, March 1909) brought up to 1913 by Professor Bowley and reduced to basis 1913=100.

‡ Money Income calculated on basis adopted by Professor Bowley (but without allowing for unemployment) and divided by Mr. G. H. Wood's Index Number of the Cost of Living (*Statistical Journal*, March 1909) brought up to date by Professor Bowley.

§ Based on Mr. Wood's Index Numbers of Consumption (*Statistical Journal*, December 1899), omitting cotton, wool, wine and spirits, weighted on System V in Mr. Wood's paper and carried forward by figures supplied by Professor Bowley. The first figure relates to 1860, 1862, and 1864. Mean of 1870–79=100.

‖ The figures give the percentage of persons living not more than two to a room in Scotland at each census from 1861 to 1911. Comparable figures for successive censuses are not available for England and Wales.

¶ 1858-64. ** 1905-12. †† 1905-11. ‡‡ 1860-64.

of five important industries (coal, pig iron, shipbuilding, cotton, wool), measured either by production or by consumption of raw material, and of our export trade as a whole; the course of 'real wages' and of 'real income,' *i.e.* of money rates of wages and of money income per head, corrected to allow for changes in the purchasing power of money; the consumption of certain articles of food and drink; and housing. The influence of the growth of the population and the influence of fluctuations in prices have both been eliminated. The figures are presented in two ways; the upper half of the table gives actual figures of production, consumption or 'real income' per head; the lower half gives the same figures as index numbers in which the figure for 1900 is taken=100 and forms the basis. Comparisons with this critical epoch are thus made easy. What does the table show?

It shows, first, for every separate index a marked and almost unbroken rise, epoch by epoch, to the last but one in 1900. There are occasional reactions (as with pig iron from 1880 to 1890 or cotton in the following decade), but these are only ripples on a powerful and rapid stream. From starting-points of about 50 or 60 the various indices moved in fifty years to 100; the general progress from 1890 to 1900 was not less than in previous decades. Unquestionably up to 1900 the average productivity and prosperity of each unit of the population rose as the number of units rose; there was a rapidly increasing return to labour as a whole. This was the complacent Victorian Age which led the world in material progress and piled up savings without effort.

The table shows, next, from 1900 to 1910 a more interesting but more dubious picture. With one exception—real wages—every index has risen, but with two exceptions—coal production and exports—the rise is slower than in previous decades, and in more than one case is barely perceptible. Running our eyes along the last three lines of the table, we see pig iron going from 93 in 1890 to 100 in 1900 and only 101 in 1910; shipbuilding goes 82, 100, 105; wool 96, 100, 102; real wages 91, 100, 100; real income 87, 100, 102; consumption of food and drink 91, 100, 102; housing 95, 100, 104. In index after index a rapid rise to 1900 is followed by a smaller rise, or by no rise at all, to 1910. In cotton there had been reaction from 1890 to 1900; the resumed progress to 1910 was at much below the former average rate. Only in coal production and exports is the rapid progress of Victorian days maintained or accelerated; those two indices represent largely one factor, not two, for coal more than anything else swelled our recent exports [5] In every other case, rapid certain growth to 1900 gives place

[5] Curiously enough coal is the product for which a diminishing return to labour in this country, not since 1900 merely but long before, seems to be most definitely established. In relation to the number of persons actually employed in mining the output has fallen rapidly, from 324 tons per head per annum in 1881-85, to 288 tons in 1895-99 and 254 tons in 1909-13. If we combine these figures with those showing the relative movement in the wholesale prices of coal and of corn, we find that the amount of corn that could be bought by one person's output of coal in a year rose 30 per cent. from 1881-85 to 1895-99, and was stationary from then to 1909-13; as the hours of work had been reduced between the two latter epochs, the real cost in mining labour of a given quantity of corn had continued to fall slightly even in Britain. The increasing response of Nature to agricultural effort was just more than sufficient to outweigh the effects of her diminishing response to the British miner.

to small and dubious improvement in the next ten years. This is the cramped, uneasy, envious, but not impoverished age of Edward.

None of the indices, indeed, records an actual decline; all still show progress however small. Even if the index of 'real wages' —stationary from 1900 to 1910—be accepted without question, the workman was slightly better off at the later epoch, since hours of work were less; he was getting the same wages for a shorter week. We cannot speak of a falling return to labour; at most we see a lower rate of increase, such as might, or might not, precede an actual fall. The contrast, however, between the Victorian and the Edwardian ages is unquestionably disturbing. In Britain, if not in Europe as a whole, the turn of the century seems to bring a turn of fortune. What conclusions are we to draw? What remedies, if any, can we apply? We shall find reasons for not being too ready to despair of the commonwealth.

The Edwardian Age and its Meaning.

In the first place, there is ground for optimistic doubts as to the figures themselves. Several of them, particularly the indices of real income, real wages and consumption, are elaborate structures based largely on estimates; others are suspect for various reasons; none need be believed to the death.[6] And even if the structure be sound, no established index of material prosperity can be expected to rise indefinitely. Progress involves change. When a nation has reached a certain point in the consumption of necessaries, it will utilise further purchasing power, not in consuming more of those necessaries, but in other ways: in buying bananas and condensed milk instead of more bread or meat, in tasting leisure, education, travel, football, cinemas, and other delights which do not appear in any index. So there may be a saturation-point in production; after putting its growing strength for many years into shipbuilding or cotton a nation may find greater need for its services in other directions—in transport, commerce, or finance.

[6] Two special causes of doubt are worth mentioning :—

(1) The presentation of the figures as averages for particular decades, necessary as it is in order to give within reasonable space a summary picture of the whole, is deceptive, because the various decades are unequally affected by the phases of the trade cycle. The years 1895-1904 contain but one year of slight depression (1904) and an undue proportion of 'good' years. The nine years 1905-13 contain the end of the slight 1904-5 depression and the whole of the exceptionally severe depression of 1908-9. The course of cyclical fluctuation unfairly weights the comparison against the later epoch.

(2) The falling off of cotton, not only in the last decade but ever since 1880, is in large part apparent only. British industry was concentrating more and more on fine counts, using more spindles and producing more value for the same weight of raw cotton.

A point on the other side, *i.e.* making the comparison unduly favourable to later epochs, is the change in the age-constitution of the population. The population in 1910 included a larger proportion of adults and a smaller proportion of children than that of 1900; production and consumption 'per head' should have been slightly higher to maintain the same standard in relation to capacity. The correction to be applied on this account is too small to disturb the comparison appreciably.

In the second place, even if we admit, as I, for one, am prepared to admit, that there was some real change in our conditions, some faltering in our progress in the first years of this century, it may yet be no more than a transient phenomenon, a result of special causes not pointing to permanent change. At the turn of the century we do in fact find special and temporary influences disturbing our ordinary development. One of these is the South African War; that war, like other wars, probably caused a greater loss of savings than of human life; it would leave capital scarce relatively to labour and in a stronger position to bargain. Another is the change in the movement of prices. Just before 1900 the falling tide of prices turned. From 1900 to 1913 we lived on a rising tide. This also is an element favouring capital as against labour, profits rather than wages. Yet another special influence at the turn of the century is a change in the rate of labour supply, due partly to the course of birth- and death-rates more than twenty years before and partly to the development of compulsory education. This point calls for explanation.

In 1876 the birth-rate in this country reached its maximum. At the same time, or just before, important steps were taken for the improvement of public health; the death-rate, which had changed little for thirty years, began to fall, and fell steadily thereafter. There followed a quarter of a century later, as a wave follows a distant earthquake, an abnormal growth in the supply of adult labour. As has been pointed out by Mr. Yule, the number of males aged twenty to fifty-five rose 19 per cent. from 1891 to 1901, as compared with a rise of 14 per cent. from 1881 to 1891, and 10 per cent. in earlier decades.[7] If we take five-year averages the rate of natural increase (difference of birth- and death-rates) reached its highest points in the years 1876-1880 and 1881-1885. Normally, this would have shown itself first by large numbers of boys entering the labour market in the early 'nineties. At the same time, however, the Education Acts were withdrawing more and more boys under fourteen into the schools. The State dammed up the rising flow of juvenile labour for a year or two. The main pressure in the labour market began to be felt later, *i.e.* about 1900, and presented itself as the 'problem of boy labour,' which was really the problem of those who had got boys' work easily enough between fourteen and twenty (replacing the younger children kept at school), but found themselves in difficulties when they reached man's estate. This abnormal movement was bound, for the time at least, to disturb the balance between the growth of capital needed to employ labour and the growth of labour seeking employment. Some temporary pressure in the labour market was inevitable. It might cause a check in economic progress as measured per head of the total population; it would certainly, in the bargaining between labour and capital for the division of their joint product, make labour for the moment relatively weak and capital for the moment relatively strong because scarce. Wages would lose relatively to profits.

[7] See Mr. Yule's paper on 'Changes in the Birth and Marriage Rates' in the Journal of the Royal Statistical Society, March 1906.

All these special influences favour capital against labour. It is in accord with them that, of all our economic indices, that which shows worst, the only one that shows no progress at all from 1900 to 1910, is real wages, the reward to labour; that which almost alone shows continued progress at the full Victorian rate is exports, to be explained perhaps in large measure as the surplus profits of capital.

With these points in mind, we reach an economic interpretation of the Edwardian age, reasonable in itself and consistent with other than economic records. That age does not live in our memories and will not live in drama and fiction[8] as a season of hard living and hard labour. It comes back to us now rather in the guise of the ball before Waterloo, as an episode of unexampled spending and luxury; as the time when we saw our roads beset by motors, our countryside by golfers, our football grounds by hundred thousand crowds and a new industry of bookmakers, our ballrooms and dining-rooms by every form of extravagance. The smooth development of Victorian days was broken, but the characteristic of the time was rather inequality of fortune than general misfortune; discontent rather than poverty; a gain by capital in relation to labour, by profits in relation to wages, by some classes of workmen at the expense of others, even more than a check to our progress as a nation. Some check to our national progress there probably was, but we are not bound to believe that the check was permanent. The three factors described above—the earthquake wave of labour supply, the South African War, and the upward turn of prices—are all peculiar to their time. The relative shortage of capital would tend to produce its own corrective. Difficulty in absorbing an abnormal flood of new labour does not prove permanent over-population; if all the hundred million persons who now find room and growing opportunities in the United States had landed there at once they would all have starved.[9]

In the last three years before the War we find in nearly all indices resumption of a rapid upward movement. What would have happened if the War had not come? Would the Edwardian age have proved a passing episode of unrest or the beginning of a serious threat to our prosperity? This is one of many questions whose answer is buried in the common grave of war.

In the third place, even if the new century was to see in Britain a lasting and not a transient harshening of conditions, if the rich ease of the Victorian age had gone for ever with Victoria, there is little ground for surprise. Malthus or no Malthus, it was not reasonable to expect Britain to keep up for ever the speed that marked her start in the industrial race. Providence had not concentrated in these islands the coal and iron supplies of all the world. As the United States and Germany and France developed their own mineral resources, Britain was destined to find her general industrial supremacy challenged, now in one field now in another; she would be driven to discover and maintain those branches of work in

[8] *Sonia*, by Stephen McKenna; *Tono-Bungay*, By H. G. Wells; *The Regent*, by Arnold Bennett.

[9] This is pointed out by a recent author, Mr. H. Wright, in *Population*, p. 110 ('Cambridge Economic Handbooks,' 1923).

which she had the greatest economic advantage, and to withdraw from the rest. This process of challenge and adjustment was bound to occur irrespective of the growth of population, and as it occurred to give rise to strains and pressures; when accomplished it might yet leave room for progress, if not at the full Victorian pace.

Of Britain before the War we may conclude that the position called for serious thought, not tears or panic. The economic records are open to diverse readings. The check to material progress in the Edwardian age may in part have been less than appears, and in part real but due to transient causes. At worst our industrial rank was challenged, not destroyed; forgetting some of the slacknesses of our easy days, we might through science and system and industrial peace have won a new lease of rapid progress. In this direction lay our remedy; in this, I think, rather than in hastening the process of birth restriction which had begun a generation before.

Britain and Austria after the War.

Let us pass to Britain after the War. Here, statistical tests of progress must be abandoned altogether. War's disturbance of our economic life and all its standards and records is barely subsiding; to found judgments of the future on the course of production or wages or prices in the years of demobilisation is vanity. Judgment by recorded results is impossible; we are driven back to general considerations for an estimate of prospects in this new but not better world.

The first principle of population to-day is that under conditions of economic specialisation and international trade the population problem in any particular country cannot profitably be considered without reference to other countries. The problem in every country is a problem of the distribution of the population of the world as a whole. The actual density in different regions of the earth varies fantastically, according to the part which that region plays in the life of the world, from less than one person per square kilometre in Canada or three in the Argentine, through 186 in Britain, or 245 in Belgium, to 760 in Monaco or 3,538 in Gibraltar.[10] The 'optimum density'[11] for any one country at each moment depends not solely or even mainly upon its own resources of natural fertility or mineral treasure, on its own achievements of technique or co-operation, but on how in each of these matters it compares with other countries, on whether other countries are prospering or depressed, on the relations of its own people—in respect of peace or war, of trade or tariffs—towards other peoples.

Britain illustrates this principle more clearly than any other great

[10] These figures relate to 1911 and are taken from Table I of the International Yearbook of Agricultural Statistics. A remarkable instance of the density possible to a purely agricultural population is presented by Java and Madura, which in 1921 had a population of 35,000,000, living 266 to the square kilometre, more than the most crowded industrial states of Europe. This involves of course a Chinese standard of life.

[11] That is, the density which will bring the largest return per head of the population. *Cf.* Cannan, *Wealth*, p. 68, and Carr-Saunders, *The Population Problem*, pp. 200 seq.

country, because of all great countries Britain has grown to be the least self-sufficient, the most highly specialised, the most dependent on trade and peace and world-wide co-operation. A pregnant analogy will make the position clear

In Central Europe, before the War, lived, under one dynastic ruler, a congeries of communities known collectively as the Austro-Hungarian Empire. These communities formed together a single economic unit, a free-trade area with fifty million inhabitants, in which every stage of economic activity, from the simplest agriculture to the most developed finance, was strongly represented, in which all the separate functions came to be distributed locally according to economic advantage without regard to internal boundaries. Some regions—east and south—were predominantly agricultural; in the north-west were extractive industries of coal and iron, and manufactures founded upon them; further south were other manufactures, and the main seat of commerce and finance. Here was timber; there water-power. Each industry tended to settle where it could most profitably be carried on. Within each industry local specialisation often went very far; thus, in cotton, one region predominated in the first and final processes (spinning and bleaching), another had more than its share of intermediate processes (such as weaving); the locomotives for railways came to be built in one region and the waggons in another. In the centre lay Vienna, a natural meeting-point entrenched by art in a system of radiating railways, concentrating on itself the most advanced stages of social life—fine manufactures, commerce, distribution, transport, finance, administration—a large and prosperous head directing and nourished by a large body. While the Austro-Hungarian Empire lasted, this headship brought with it the first place in prosperity. The wealth, pleasure, and extravagance, no less than the government, education, science and art, of fifty millions made Vienna their centre.

The War came and went, and with it went the Empire. The dynastic ruler disappeared; the congeries dissolved; each community became a separate body desiring and needing a separate head, aiming at self-sufficiency, seeking it by economic barriers against intercourse. In that break-up the average prosperity of all the fifty millions has sunk. Nearly every region is in some way poorer than before. But no region has suffered as much as Vienna; in none does the loss take the characteristic appearance of over-population. Vienna remains a head grotesquely too large for the shrunken body of German Austria, manifestly over-populated, as little able to support its former numbers at their former standard, as would be Monaco if the nations gave up gambling or Gibraltar if they gave up war. It is over-populated, not through exhaustion of its natural resources, not because in the past its people were too prolific, but because the world outside has changed too suddenly.

De nobis fabula—the fate of German Austria is the moral for Britain. No other country of comparable size is so highly specialised as Britain. None produces so small a proportion of the food that it requires, or of the raw materials of its industries. None is so predominantly engaged in the advanced ranges of economic activity;

in industry rather than agriculture; in finishing processes rather than the extraction of raw material; in transport, commerce and finance, rather than manufacture. No other country, therefore, is so completely dependent upon the restoration of peace and trade and economic co-operation. None is destined to suffer so acutely from any general disorder. At this moment perhaps none is suffering so much.

It is needless to seek in excessive fecundity an explanation of our present troubles. There are other reasons, enough and to spare, why we should expect now to suffer from unexampled unemployment. Two exceptional causes of unemployment are now added to the normal movement of cyclical fluctuation. One is the difficulty of passing from war and war industries to peace—the difficulty of making swordsmen into ploughboys. The process of training and directing the new supplies of labour to fit the changing needs of industry has been broken by the War; there is a maladjustment of quality between labour supply and labour demand. The second cause lies in the damage done by the War and its aftermath to the economic structure of the world; the destruction of capital, the relapse of great nations towards barbarism, the breaking of easy and friendly intercourse, the continuance of war measures, the smaller volume of international trade and its shifting into new channels. The world has changed suddenly, if less completely, round us as round German Austria. Many of our trades find their former customers dead or impoverished or cut off by new barriers; the labour trained to those trades cannot shift to fill the gap in production which is left by the disappearance of those customers and their work. In both these ways, in terms which I used in writing of unemployment fifteen years ago, we have leading instances of those 'changes of industrial structure' which leave legacies of enduring unemployment, to be reduced only as the labour ill-fitted for new needs is slowly and individually absorbed again or is removed by death or emigration.[12]

The fate of Austria has a bearing not on war alone. The world may change otherwise than by war. The 'optimum density' of population for any country may be diminished not by anything happening in that country, but by the discovery and exploitation of resources in other countries; possibly even by tariff changes. The more any country is specialised in its economic functions, above all if it is specialised in the most developed rather than in the primary functions, the greater is its liability to such changes. Britain, becoming yearly less self-sufficient, setting each year a swiftly growing people to more and more specialised labour, increasing each year its inward and outward trade, was before the War taking more and more the Austrian risk. It is arguable that with this lesson before us we ought no longer to take the risk so fully; should retrace our specialisation and aim at self-sufficiency—in practical terms, under a system of tariffs or bounties,

[12] Uncertainty as to the course of prices, with its paralysing effect on business enterprise, ought perhaps to be named as yet another special cause of post-war unemployment. Alternation of upward and downward movements of prices is, of course, one of the elements in normal cyclical fluctuation.

should grow more corn and do less trade. The practical answer to that argument is that we are already too far from self-sufficiency to make worth while any attempt to return. Any change great enough to diminish seriously our dependence on overseas trade, in other words our exposure to the Austrian risk, would involve an impracticable reduction in our total population and our average wealth. A middle course that is sometimes suggested is to aim at self-sufficiency in the British Empire, by tariff arrangements favouring Imperial rather than foreign trade. The adoption of such arrangements clearly depends more on the wishes of the Dominions than on those of Britain, and their value for the purpose in view upon the readiness of the Dominions to acquiesce in a division of economic functions which would leave the most advanced and most profitable ones to the British Isles. It is more than doubtful whether this is the Dominion view of Imperial economics. In the last analysis, the long road which Britain has travelled to dependence on international trade, as general and as free as possible, will, I believe, be found to be irretraceable. Like the hero of one of Mr. Wells' novels, the Britain that we know, the Britain of forty millions, has been made for a peaceful and co-operative world; she must try to create such a world if she does not find it ready to hand.

Recapitulation.

Let me try to gather together the threads of this long discussion. A further quotation from Mr. Keynes' writings will serve for a starting-point:—

'The most interesting question in the world,' he writes, '(of those at least of which time will bring us an answer) is whether, after a short interval of recovery, material progress will be resumed, or whether, on the other hand, the magnificent episode of the nineteenth century is over. In attempting to answer this question it is important not to exaggerate the direct effects of the late War. If the permanent underlying influences are favourable, the effects of the War will be no more lasting than were those of the wars of Napoleon. But if even before the War the underlying influences were becoming less favourable, then the effects of the War may have been decisive in settling the date of the transition from progress to retrogression.[13]

The warning deserves attention. Yet, as I am less inclined than Mr. Keynes to be pessimistic about the tendencies before the War, I feel perhaps more pessimistic than he is in this passage about the effects of the War, and the possibly enduring damage it may have done and be destined to do to humanity. Another criticism that may fairly be made upon this passage and the paper from which it comes is that in neither is it clear how large an area is referred to, whether Britain or Europe or the world.

Before the War, as I have tried to show, there is nothing to suggest that Europe had reached its economic climax; Malthus' Devil, unchained again or not, cannot be found where Mr. Keynes professes to find him.

[13] 'An Economist's View of Population,' in the Manchester Guardian Reconstruction Supplement, Section Six (1922).

For the world of white men as a whole there is even less ground for pessimism; the limits of agricultural expansion are indefinitely far. If we regard only that part of this world which is known as Britain, judgment is not so easy. Some change did come over our economic life, or certain parts of it, with the turn of the century; our effortless supremacy was challenged. Reasonable men may dispute, and since the decisive evidence has perished will probably dispute for ever, whether the unrest and uncertainty of the Edwardian age marked a passing episode destined but for the War to give place to a fresh stage of swiftly rising prosperity, or, on the other hand, recorded the first shock of permanent forces working to make life in these islands less easy and to set a term to material progress.

After the War—for that phase, if indeed we have reached it, I doubt whether we may find much comfort in Napoleonic parallels. The Napoleonic wars were wars between Governments and armies rather than peoples; they did not bite deeply into economic life; they left it possible for the best contemporary fiction to show a picture of English society in which the military figure chiefly as dancing partners.[14] The war of 1914-18 was waged on millions of non-combatants, as much as on armies; it is being continued in the same form to-day; the economic structure of the world, battered out of shape by four years of open war, is still twisted by human passions. The lesson of compulsory self-sufficiency has been learnt too well; in all parts of the world, by new economic barriers, nations are endeavouring to safeguard, at the expense of their native and natural industries, the industries which were forced on them by the extremities of war. The world is poorer in resources by its lost years and ruined capital; of those diminished resources it makes worse use.[15]

To sum up, for Europe and its races the underlying influences in economics were probably still favourable when the War began. But the war damage was great and we are not in sight of its end. Man for his present troubles has to accuse neither the niggardliness of Nature nor his own instinct of reproduction, but other instincts as primitive and, in excess, as fatal to Utopian dreams. He has to find the remedy elsewhere than in birth control.

The Population Problem Remains.

Let me add one word of warning before I finish. Examination of economic tendencies before the War yields no ground for alarm as to the immediate future of mankind, no justification for Malthusian panic. This negative conclusion does not discredit the fundamental principle of Malthus, reinforced as it can be by the teachings of modern science. The idea that mankind, while reducing indefinitely the risks to human life, can, without disaster, continue to exercise to the full a power of reproduction adapted to the perils of savage or pre-human days, can

14 Jane Austen's first three novels were written during the Revolutionary Wars (1796 to 1798); her last three between Wagram (1809) and Waterloo (1815).

15 The recent development of prohibitive tariffs is very fully described in a special supplement by Dr. Gregory to the London and Cambridge Economic Service.

control death by art and leave births to Nature, is biologically absurd. The rapid cumulative increase following on any practical application of this idea would within measurable time make civilisation imposible in this or any other planet.

In fact, this idea is no more a fundamental part of human thought than is the doctrine of *laissez faire* in economics, which has been its contemporary, alike in dominance and in decay. Sociology and history show that man has hardly ever acted on this idea; at nearly all stages of his development he has, directly or indirectly, limited the number of his descendants.[16] Vital statistics show that the European races, after a phase of headlong increase, are returning to restriction. The revolutionary fall of fertility among these races within the past fifty years, while it has some mysterious features, is due in the main to practices as deliberate as infanticide. The questions now facing us are how far the fall will go; whether it will bring about a stationary white population after or long before the white man's world is full; how the varying incidence of restriction among different social classes or creeds will affect the stock; how far the unequal adoption of birth control by different races will leave one race at the mercy of another's growing numbers, or drive it to armaments and perpetual aggression in self-defence.

To answer these questions is beyond my scope, as it is beside my purpose to pass judgment on the practices from which they spring. The purpose of my paper is rather to give reasons for suspending judgment till we know more. The authority of economic science cannot be invoked for the intensification of these practices as a measure for to-day. Increased birth control is not required by anything in the condition of Europe before the War, and is irrelevant to our present troubles. But behind these troubles the problem of numbers waits—the last inexorable riddle for mankind. To multiply the people and not increase the joy is the most dismal end that can be set for human striving. If we desire another end than that, we should not burk discussion of the means. However the matter be judged, there is full time for inquiry, before fecundity destroys us, but inquiry and frank discussion there must be. Two inquiries in particular it seems well to suggest at once.

The first is an investigation into the potential agricultural resources of the world. There has been more than one elaborate examination of coal supplies; we have estimates of the total stock of coal down to various depths in Britain and Germany, in America, China, and elsewhere; we can form some impression of how long at given rates of consumption each of those stocks will last; we know that 'exhaustion' is not an issue for this generation or many generations to come. There has been no corresponding study of agricultural resources; there is not material even for a guess at what proportion of the vast regions—in Canada, Siberia, South America, Africa, Australia—now used for no productive purpose could be made productive; at what proportion of all the 'productive' but ill-cultivated land could with varying degrees of trouble be fitted for corn and pasture. Without some estimate on such points, discussion of the problem of world population is mere

[16] See *The Problem of Population*, by A. M. Carr-Saunders.

groping in the dark. The inquiry itself is one that by an combination of experts in geographic and economic science—commission gathering opinions or an office gathering returns—it should not be difficult to make.

The second is an investigation into the physical, social effects of that restriction of fertility which [illegible] become a leading feature of the problem. This also is a matter neither for one person—for its scope covers several sciences—nor for a commission; facts rather than opinions or prejudices are required.

If the question be asked, not what inquiries should be made but what action should now be taken, it is difficult to go beyond the generalities of reconstruction, of peace and trade abroad, of effic[illegible] and education at home. The more completely we can restore the nomic system under which our people grew, the sooner shall we [illegible] them again in productive labour. Unless we can make the world again a vast co-operative commonwealth of trade, we shall not find it spacious enough or rich enough to demand from these islands the special services by which alone they can sustain their teeming population. Even if the world becomes again large enough to hold us, we shall not keep our place in it with the ease of Victorian days; we dare no longer allow, on either side of the wage bargain, methods which waste machinery or brains or labour. Finally, if there be any question of numbers, if there be any risk that our people may grow too many, the last folly that we can afford is to lower their quality and go back in measures of health or education. Recoil from standards once reached is the gesture of a community touched by decay.

TRANSPORT AND ITS INDEBTEDNESS TO SCIENCE.

ADDRESS BY

SIR HENRY FOWLER, K.B.E.,

PRESIDENT OF THE SECTION.

I FEEL that it is right that the Engineering Section of the Association here in Liverpool should devote one of its sessions to the subject of traction. There is no city in the Empire, or in the world, which is so dependent on traction in one way or the other as the one in which we are meeting to-day, and I can also say without fear of contradiction that there is no city in the world which has acted as so great a pioneer in traction development as this one on the Mersey.

Its very birth was caused by the physical features it presented at a time when the estuary of the Dee was silting up, whilst whatever may be the derivation of the first portion of its name, there is no question but that the latter portion refers to the advantages it offered for water transport.

It is not necessary, nor am I qualified, to speak of the development of the 'pool' into the port which means so much to Liverpool at the present day, but there are other methods of transport in which it has played an important part that I should like to mention.

As early as 1777 Liverpool realised the necessity and advantages of easy and cheap transport, and the canal from Liverpool to the Trent was constructed at that date, having a length of ninety-six miles. This joined the Trent at Shardlow, not far from Nottingham, and it has recently been suggested the river should be canalised from there to the sea on the East Coast.

More recently Liverpool has become connected with its sister city of Manchester by the Ship Canal, in the carrying out of which many interesting engineering problems were met and solved.

The better remembered event is, however, in connection with transport by rail. It was the construction of the Liverpool and Manchester Railway in 1829 and its immediate success that more than anything else impressed on the country the fact that a new system of traction was opening out unheard-of possibilities. It is not too much to say that the production of the 'Rocket' for the trials at Rainhill in October 1829 marked the first step in the practical commercial success of railways.

This, however, has not been the last association of the city in pioneer work on the rail in this country. In 1904 the Liverpool and Southport section of the Lancashire and Yorkshire Railway was electrified, this being the first inter-urban electric line in this country. The change was due to the enterprise and foresight of Mr. (Sir) John A. F. Aspinall,

a distinguished son of Liverpool, and the Directors of the Lancashire and Yorkshire Railway. The electrification of the line was preceded by exhaustive trials to determine the tractive force required to overcome the resistance on railways,[1] and with these trials I had the honour of being connected.

The other matter in which Liverpool has done pioneer work on traction is that of heavy motor traffic. From its inception in 1895 the Liverpool Self-propelled Traffic Association was specially connected with this method of transport. Under the presidency of the late Sir Alfred Jones, with the guidance of Dr. Hele Shaw and under the organising ability of its enthusiastic and energetic secretary, Mr. E. Shrapnell Smith, it organised and carried out trials of commercial vehicles in 1898, 1899, and 1901. In May 1898 were carried out the first practical trials of these vehicles held in the country, and I had the honour of being the observer of the first lorry to leave the yard. The Motor Car Act of 1903, which allowed a practical weight for commercial road motor vehicles, was the result of a deputation of the Liverpool Self-propelled Traffic Association waiting on the President of the Local Government Board (the Right Hon. Walter Long, now Viscount Long) when he was on a visit to Liverpool.

I think I have said enough to justify the statement I made that it is fitting that one of our sessions here in this city of Liverpool should be devoted to the question of transport, and I wish to speak of its indebtedness to Science, and trust I may be able to show that, as with other branches of engineering, its progress is due to science, and, in concluding, speak of how it may repay, if inadequately, the debt under which it is placed.

We are perhaps too apt at the present time to forget the obligation which the world owes to transportation, so commonplace have the improved methods become. We are already forgetting the lesson that the submarine menace gave us on this matter during the War, and again looking upon the movement of matter from point to point as a commonplace occurrence. It has been said that effective transportation is one of the great aids to civilisation, but it must not be forgotten that all movement of material from place to place is economically waste as far as the dissipation of work is concerned. Problems of transportation have been solved more or less successfully in all ages, and some of them, such as the moving of the stone to Stonehenge, &c., still excite our wonder and admiration. Such works, and similar ones of much greater magnitude in the East, however, we feel as engineers could be accomplished by quite crude methods if there was unlimited labour available, and if time were of no consequence.

The transportation which aids civilisation is that which cuts down the wastage of power to a minimum and which reduces the time occupied in carrying this out. It is here that science has helped in times past, and will help increasingly in the future if we are to go forward. In no other branch is Telford's dictum that the science of engineering is 'the art of directing the great sources of power in Nature for the use and

[1] See Mr. (Sir) J. A. F. Aspinall's paper on 'Train Resistance,' *Proceedings of the Institution of Civil Engineers*, vol. 147, 1901.

convenience of man' so well exemplified, and this utilisation has been carried forward at ever increasing speed during the last hundred years.

If we take the definition of Science as 'ordered knowledge of natural phenomena and of the relations between them,' as given by W. C. D. Whetham in the 'Encyclopædia Britannica,' we shall easily see how transportation has been dependent upon it.

It may be that some may not agree with this definition of 'ordered knowledge of natural phenomena,' but I feel that after thought it will be recognised that it covers very completely what we call Science. We are rather apt to confuse the knowledge with the means and apparatus applied in getting it. Recently I have read an article which called attention to the dependence of science upon engineering or mechanical achievement, but surely the accuracy we get, the lack of which was such a great drawback to the investigations of a century to a century and a half ago, is itself based upon 'ordered knowledge.'

Dealing with transport, it may be said roughly that it is mainly dependent upon three things—the method of propulsion, the material available for use, and the path over which traction takes place. I cannot deal fully even with one of these, and propose to confine my remarks to the first two, which are the ones I am best acquainted with.

It may be said that advance in traction really became rapid when methods of propulsion other than those of animals and the force of the wind became available. The greatest step forward—wonderful as some of the achievements of aeronautics have been of recent years—came with the development of the steam engine.

Like most great achievements in the world, it was not a lucky and sudden discovery of one individual, although here as elsewhere we associate the work with the name of one man especially. This has usually been the case, and without wishing to detract from the work of the individuals who are fortunate enough to utilise the ordered knowledge available to the practical use of man, one must not forget the labours of those who have sought out that knowledge and have given it freely to the world, thus placing it at the disposal of the one whose imagination and creative faculty were great enough to see how it could be utilised in the service of man.

The first attempt at traction by using a steam engine was a failure because of the lack of this knowledge. I refer to the work of Jonathan Hulls and his attempt in 1736-7 to apply one to the propulsion of a boat on the River Avon in Worcestershire. He failed because of the lack of that knowledge, although undoubtedly he possessed the necessary imagination.

Although James Watt is not directly associated with traction, it was his application of science to practical use that finally gave the greatest impulse to transportation that it has ever had. No advance had taken place to Newcomen's engine of 1720 until Watt's work of 1769. His knowledge of Black's work at Glasgow on the latent heat of steam and his own experiments with the Newcomen model led to the success of his improvements of the steam engine. His scientific knowledge is clearly shown in his patents and publications, for he dealt with steam jacketing in 1769, with expansive working in 1782, and he devised his parallel motion in 1784. His direct connection with transport includes the reference to a steam carriage and a screw propeller in 1784, whilst

the firm of Boulton & Watt corresponded with Fulton for a period extending from 1794 to 1805.

Although Cugnot in 1770 and Murdoch in 1786 had made models of vehicles propelled by steam, it was Richard Trevithick with his steam carriage in 1801 and 1803 and ill-fated railway in 1804 who first showed the practical application which could be made. It is probable that the engine which his assistant, Steel, took to the wagon-way at Wylam in 1805 turned the thoughts of George Stephenson to the work that has meant so much for us. No one can read the early life of the father of railways without appreciating that he was from young manhood a searcher after scientific knowledge. Doubtless he owed much to the friendship of Sir William Fairbairn, the President of our Association in 1861. The advances he gave to the world of transport were all due to his practical application of the knowledge he had obtained himself or had learned from others. It is so often thought that because the early inventors and engineers of the beginning of last century had not received what we now call a scientific education that they were not in any sense of the term men of science. It must be remembered that at that time the knowledge of natural phenomena was very limited, and it was possible to know much more easily all the information available on a subject than at the present day, when we have such a mass of miscellaneous information to hand on every conceivable subject. It was ordered knowledge which led Stephenson to adopt the blast-pipe of Trevithick. It was the desirability of obtaining ordered knowledge that caused him to carry out those experiments which showed to him the advantages of using rails, and it was the scientific appreciation of the necessity of increased heating surface that made him adopt the suggestion of using tubes through the water space in the boiler of the 'Rocket.' His appreciation of the advantages of science was shown by his acceptance of the Presidency of the Mechanical Science Section (then as now Section G) of our Association in 1838. It is interesting to note that one of the earliest grants in Section G was for a constant indicator (for locomotives) and dynamometric instruments in 1842-43, whilst Stephenson was still alive. Let me remind you of his ready grasp of the application of a known principle to a different object by the story of the invention of the steam whistle. On the Leicester and Swannington Railway, which followed the Liverpool and Manchester, one of the Newcastle locomotive drivers—R. Weatherburn—at a level crossing ran into the cart belonging to an old lady, destroying her eggs and butter. Upon his return to Leicester, and reporting this to Stephenson, he was at once told to go down the town to a trumpet-maker and get him to make a trumpet which could be blown by steam. None but a mind in which the knowledge of natural phenomena was very carefully ordered could have so readily solved such a problem.

From the time of Stephenson the progress in propulsion on rails by steam locomotives was steady if slow. The investigations for a long while were largely confined to the question of expansion and condensation, and although the results attained were noteworthy in the case of steamships, on the rail—to which for the moment I will confine myself—there was little advance in the principle of propulsion, but, as I shall show later, the improvements in materials allowed a steady growth in

power and size. Although work was done by compounding and using higher pressures, the greatest advance has come to steam locomotives by the use of superheated steam. This was no new thing, for Papin in 1705 seemed to have an appreciation of its value. As pressures and the resultant temperatures increased there came difficulties with lubrication. With the increased use and knowledge of mineral lubricants Dr. Schmit was in 1895 able to devise methods of using superheated steam which have been of the greatest use to transport and to the community.

The progress of transport on the rail has latterly strongly followed other lines, and I must for a few minutes go back again to the development of the use of steam in a turbine in order to speak of the subject of electric traction

In spite of the fact that the idea of the utilisation of steam for giving rotary motion is old, its commercial adaptation in the turbine is modern. Rarely, if ever, has there been such a direct and instantaneous application of science to practice. We are too close at present to the matter to realise what a change has taken place in the world owing to the introduction of the steam turbine.

If we think for a moment we shall realise what a change has come over our lives, not only in an engineering but in a general sense, since the end of last century. It has truly been said that this is very largely due to an Italian experimenting with Hertz waves, to numberless young men lying on their backs on muddy roads under motor-cars, and not least to a young Irish engineer who revolutionised transport.

One realises the work done by De Laval, Curtiss, Rateau, and the brothers Ljungstrom, but the name which will always be associated with the steam turbine as firmly as that of James Watt is with the inception of the steam engine is that of Sir Charles A. Parsons, our President for the Meeting of 1919 at Bournemouth. The success of his work is due to his application of scientific principles to the many points of the turbine and its accessories. Apart from its application to marine work, it is the turbine which has made possible the economical production of electrical energy, which is doing so much, and will do so much more in the future, for rail transport. To-day it may be said, as it often has been, that there are no mechanical or electrical difficulties in the electrification of railways, the only difficulties being financial ones, although one could hope that the induction troubles could be overcome by a cheaper method than at present available.

It is impossible here to trace the development of electrical science from the experiments described by Gilbert in 1600 to the equipment of electric locomotives on the railways of Switzerland and the United States of America. If we were able to trace this development we should see that it has been not only a gradual but a continuous and ordered increase of knowledge of natural phenomena. One must mention, however, what a change electrical traction by train and tube has made to our town life. It has rendered our large towns possible and given a chance to millions of our workers of a wider outlook on life and the opportunity of living amongst healthier and more pleasant surroundings. This, as just stated, is not the result of a sudden discovery of some fundamental principle, but to a studied advance, step by step, from very elementary knowledge to the information we have available and at our disposal

to-day. This is very largely the result of endless laboratory research and experiment.

The last method of propulsion that I can deal with is that by means of the internal-combustion engine. This, as we almost universally have it to-day, is the result of the cycle adopted by N. A. Otto in his gas engine in 1876. Here again the engines we have to-day are the result of careful and studied investigation. It may be truly said that the advance made has been so much more rapid than in the case of the steam engine and electrical machinery because of the more advanced state of scientific knowledge, and it furnishes an example of the assistance which this gives to progress.

In relation to transport the work has proceeded on two distinct lines, the Daimler and the Diesel engines. In 1885 Gottlieb Daimler produced the engine that is associated with his name, and which utilises a light spirit which supplies a carburetted air for the explosive mixture for the cylinder. The development of this engine has itself proceeded in two directions. In the one it has been made very much more flexible and silent in its adaptation to motor-car work, whilst in the other the great desideratum has been lightness and in association with the improvements in the necessary materials has rendered possible the aeroplane as we have it to-day. In both cases the development to the degree reached has been due to a careful study primarily of the pressures, compression, and composition of the mixture.

The Diesel engine was invented in 1894 by Rudolph Diesel, and consists of the injection of oil or pulverised fuel into the engine cylinder. Its development has taken place both on the four- and two-stroke cycle, and although considerable progress has been made with land engines, it has chiefly been used for marine transport.

The internal-combustion engine has not been largely used for rail transport owing to its comparatively high cost of fuel per horse-power and its lack of flexibility. The latter is particularly the case when one remembers the high torque which is so desirable, and which can be attained in both the steam and electric locomotives in starting.

Throughout these remarks on methods of propulsion I have dealt with the points connecting them with rail transport as they occurred, as this is not only the method with which I am most familiar, but is the oldest means of using mechanical power. I must, however, say a few words as regards transport by sea, road, and air in connection with methods of propulsion.

I have already spoken of the early efforts of Hulls, and it was only natural that the work of Watt on land should be followed by application of the new power available to propulsion on the water. Although the growth after the work of Symington, Fulton, and Bell may have seemed to be slow, it was continuous, and constant experiments and research were made both in marine engines and in their application. Saving of fuel has played a much more important part here than with the locomotive, whilst more space being available and greater power required, the advantages of the expansion of steam were rendered more imperative and had greater scope than in the other long-established method of mechanical transport. The great advance came with the turbine, and it is interesting to notice that whereas in early days engines were geared

up, most of them now are geared down to the screw. Scientific methods have been applied to all those details of measurement and experiment that have led to transport by sea being carried on at increased speed and with decreased cost per ton carried. The application of liquid fuel and the introduction of Diesel engines, both with the object of increasing the space available for cargo, have been carried out on true scientific lines.

Of transport by road it may be said that its commercial inception came at a time when scientific knowledge was well advanced, and its progress was in consequence more rapid. It must not be forgotten that in the fairly early part of last century considerable work was done on scientific lines with steam cars, only to be abandoned when legislation made its continuance impossible. The development of the motor-car engine from the small unit of Daimler to the present car is undeniably due to the use of 'ordered knowledge' of the gaseous mixture, of its ignition, of the fuel itself, and of the compression that should be employed. Here again we have a case of the careful application of the principle developed with ever increasing care until we get engines as noiseless, as efficient, as reliable, and as flexible as we have them to-day. It is a case, too, where the development is so recent that many of us can remember the scorn and distrust that this method of traction excited even here in this city that was so prominent in its inception twenty-five years ago.

Very much more could be said as to the indebtedness of aeronautics to science, but the fact that this indebtedness is so self-evident, as well as the question of space at my disposal to deal with a subject of such a size, make it impossible to attempt to do justice to this part of my subject. I will only speak of the aeroplane, and its development has been even more rapid than that of the motor-car. I personally feel this when I remember that Mr. A. V. Roe was one of my students here in Lancashire in the 'nineties.

It was not until the development of the internal-combustion engine that the matter became a really practical one. The early work of Santos Dumont, Henry and Maurice Farman, Wilbur and Orville Wright, A. Vernon Roe, Cody, Rolls. Blériot, Paulhan, and others led to the close scientific consideration of the whole problem.

Step by step investigations have led towards the perfecting of this type of transport. In all cases the developments have followed careful scientific research. Amongst our fellow-countrymen the work of Rolls, Godden, Cody, Busk, Keith-Lucas, Hopkinson, Pinsent, and others has unfortunately been terminated by their deaths in the cause to which they were devoting their lives. In no other field has scientific work demanded so great a toll. This must be so when one is dealing with transport in such a medium as air. The work of others, such as—to name but a few—Bairstow, De Havilland, Sopwith, Barnwell, Handley Page, B. M. Jones, and O'Gorman, has fortunately continued. The War was naturally a great incentive to the advancement of our knowledge of aeronautics, and I feel proud that at Farnborough, at the Royal Aircraft Factory, I was allowed to be associated with such men as Aston, Dobson, Farren, Gibson, Green. Grinstead, Hill, Irving, Linderman, Thompson, and McKinnon Wood.

These were scientific men working on scientific lines, and their work was put to full practical test at once. The mass of information collected and used has been immense. One cannot in any collection of names omit one to whom one must ever be grateful—Sir Richard Glazebrook, again a son of Liverpool, who not only as Director of the National Physical Laboratory, but also as chairman, under the presidency of Lord Rayleigh, of the Advisory Committee of Aeronautics, did so much towards the development of this method of transport.

It is impossible to touch more than in the lightest possible manner on the developments which have taken place in aeronautics due to scientific work. In the means of propulsion research has given an engine of such size and so light in weight per horse-power that what was a laboured struggle against the effects of gravity has changed into the ability to rise at considerably over 1,000 feet per minute to heights where the rarefaction of the atmosphere renders it necessary for oxygen for breathing to be obtained artificially. The safety of flying as the result of the work of Busk has rendered the machines stable even in such a medium as the air. There is no greater instance of the indebtedness of transport to science than the rapidity with which the possibilities of transport by air have advanced. That the realities have not advanced at the same rate is due to financial reasons. As a rule we have a close relationship between these two, but in this instance owing to the demands of war this has not been the case, for we have the knowledge before we are financially able to use it to the greatest advantage.

The other point I would deal with in some detail is the question of materials. Here we are dealing with a matter which has to be considered in an entirely different manner. We to-day have no basic metal or material which was not known when transport first turned to mechanical methods for assistance. The change which has come about has been as largely due to the advances made in metallurgy as to the inventions in mechanics that have led to the improvements in means of propulsion and in machinery. I am aware that neither of these would have been of any use were it not for the increase in facilities of production, but most certainly the scientific work of the metallurgist is one of the many points which, taken together, have caused the resultant progress. The early builders of steam engines were not only troubled through inability to get their engines machined properly, but also with the difficulties of obtaining suitable material for the parts they required. Steel has been known for thousands of years, but its rapid and economic production is of very recent growth. It has very truly been said that every great metallurgical discovery has led to a rapid advance in other directions. I will as before deal with the railway as an example. We can hardly appreciate at this date the conditions which existed from a metallurgical standpoint on our railways when our first Meeting at Liverpool was held in 1837. Iron—made laboriously, heterogeneous in character and expensive of production not only in money but, owing to the heavy character of the methods employed, detrimental to the very character of the workman—was the only material available. Remember for a moment that this was not only the material employed for the various parts of the mechanism of the locomotive, but for the rails. However improved the methods of manu-

facture were, there could never have been a universal development of rail traction if it had depended upon material made in such a way. We are especially interested in the manner the growing demand was met. for it was at the Cheltenham Meeting of the Association in 1856 that Bessemer made public the invention he had already been working on for two years, and which was to insure a cheap method of production of a material so essential to transport. One should mention with Bessemer the name of Mushet, whose work helped so materially in getting rid of the red shortness which in the early days gave such trouble. We are apt at the present day, I am afraid, to somewhat belittle the work of Bessemer in view of the more improved methods now employed, but his name must for ever stand out as the one which made cheap transport possible. After the use of manganese in one form or the other as a deoxidiser and a 'physic' for sulphur, there, however, still remained the baneful effect, due to phosphorus, which prevented the use of the ores of more general occurrence. There have been few more epoch-making announcements made at meetings of technical subjects—although this was not appreciated at the time by many of the audience—than S. G. Thomas's announcement of the discovery of the 'basic' process, which he made at the meeting of the Iron and Steel Institute in March 1878. I say advisedly that many did not appreciate the news, for an old friend of mine who was present was impressed by the earnestness of the remarks of Thomas and the little notice taken of the short statement made. His work, associated with that of his cousin, Gilchrist, was the result of close scientific research.

Another investigation which has given great results in transport has been the ever growing use of alloy steels. For the scientific inception of these we owe a great debt to Sir Robert Hadfield, whose inventive genius and scientific mind are still active in that field he has made so particularly his own. His first investigations materially affect transport to-day. It is true that Mushet had previously worked on self-hardening tool steel containing tungsten, but the work was only carried out on a small scale. In 1882 Hadfield had produced manganese steel.[2] This is a most remarkable product with its great toughness, and is extensively used for railway and tramway crossings, where resistance to abrasion is of great value. This was the first of that very remarkable series of alloys about which I must say a few words, for they have made possible the motor-car and the aeroplane as we have them to-day. Continuing his investigations, in 1889 Hadfield produced the compound of iron and silicon[3] known as low hysteresis steel. Indirectly this is of the greatest interest from a transport standpoint, as when used in transformers it not only reduces the hysteresis losses, but allows of a considerable saving in the weight of core material.

From these early uses of alloy steels there has grown up a large number of various alloys, many of which are of the very greatest use for various transport purposes. It is not too much to say that the modern aeroplane is the result of the material now at the designers' disposal both for the engine and for the structure itself. The strength

[2] *Inst. of Civil Engineers*, vol. 93, 1888.
[3] *Iron and Steel Institute*, p. 222, Pt. II, 1889.

of some of the chrome-nickel steels combined with their ductility is extraordinary, and is due not only to the composition of the metal, but to the results which have been obtained by patient scientific investigations relating to their heat-treatment. Taking one other example, one may quote the use of high chrome steel—for the early investigations into which we owe so much to Brearley, and to its later developments to Hadfield also—for the valves of aeronautical engines, subjected as they are to high temperatures. At one time it looked as if the advantages which follow high compression and its resultant high temperatures might be lost owing to the inability of ordinary steels to resist this heat, but the employment of 13 per cent. chrome steel allowed work in this direction to be continued. Not only the aeroplane but the motor-car are, as has previously been said, the result of the work done on alloy steels.

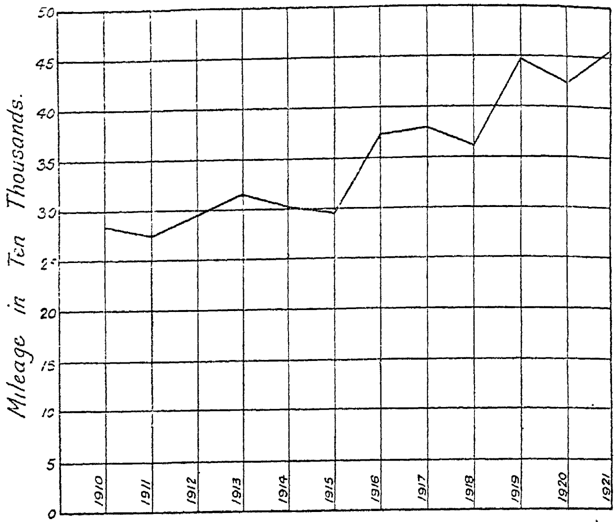
It is not only with steels that we have been benefited so much from research. The case is as marked with light alloys, which have aluminium as a base. The latter itself is the result of investigation along scientific lines, and in aeronautical work particularly much has been done towards giving a metal both light and strong by the work of Walter Rosenhain, F. C. Lea, and others.

It may be said that all I have dealt with up to the present has been the result of special investigation, and that 'ordered knowledge' is not of assistance to an everyday engineer such as myself. I may perhaps be forgiven if I refer to some personal work where the collection of that knowledge with the assistance of my colleagues, especially L. Archbutt and H. A. Treadgold, has been of great assistance to that large transport institution, the Midland Railway, with which we were so long associated. I have dealt briefly with the subject in a general way in a paper I read a little while ago before the Institution of Locomotive Engineers,[4] but would like to speak of it in more detail and in view of the fresh information that is now available. I would first speak of the results obtained with solid locomotive crank axles. Here we have a large mass of metal which in the rough state weighs about 40 cwt. It is forged from the ingot into a block about 25 in. by 18 in. in section, and this is then worked down at the two ends and in the middle to about 11 in. in diameter, the pieces of the original section of the block remaining being the throws which are twisted to an angle of 90° to each other. A block about 1½ in. thick is slotted out of each web, and from these the tests to which the crank is subjected are taken. Sometimes a crank has to be taken out of service owing to the journal wearing down below a diameter at which it is judged safe for them to run, but more often flaws are developed which, however, are progressive, and with ordinary examination can be detected before any risk is taken in running. A crank axle is an expensive portion of a locomotive, and its replacement is not only costly but takes a considerable amount of time, as the driving-wheels have to be removed and replaced. These considerations have led us to give a good deal of attention to this piece of mechanism on what we believe to be scientific lines. Careful note has been taken not only of the mechanical tests made on the portion removed from the

[4] *Inst. of Loco. Engineers*, vol. 12, 1921.

throws, but of the micro-structure of the metal itself. The first question which rises in our mind is why the cranks develop flaws at all. It is, of course, known that with ordinary structures one is able to calculate the stresses in them, but this is not so with a locomotive crank axle. Not only is it being subjected to the stresses set up by revolving it while it is loaded with the weight of a portion of the locomotive on its axle-bearings and by the steam pressure on the pistons transmitted to the crank-pins, but it has to withstand the shocks set up by its running on the rails, which cannot be calculated. These include the pressure set up on the edge of the wheels when entering a curve at a speed other than that which the super-elevation is allowed for, running over uneven rail joints and crossings, and also what I believe is one of the worst, if infrequent, the striking of check rails. These stresses and the resultants of them are most severe at the corners of the crank-pins and at the radii where the webs or throws join the rounded portions of the axle. These are the points at which flaws usually occur.

For about twenty years we have endeavoured to get the knowledge we have obtained into an ordered state, from observation and discussion with the metallurgists attached to the various manufacturing firms. Certain points are obvious, such as the necessity of a good micro-structure, and whilst the details in connection with exactly what micro-structure is the best are somewhat uncertain and open to debate, we can with confidence say that the steel 'shall be as free as possible from non-metallic enclosures, and that the micro-structure should show uniformly distributed pearlite in a sorbitic or very finely granular or lamellar condition and be free from any nodular or balled-up cementite. It must also be free from any signs of segregation and from any coarse or overheated structure.' (Extract from Midland Railway specification for crank-axle forgings.) The dimensions I have given of the size of the block of metal from which the axle is made show that it cannot have received much work, and therefore any non-metallic enclosures present will be only slightly drawn out, and will not occur as threads as they do in bars of small diameter and even in steel tyres. One of the first observations we deduced was that the life of the crank in miles had a direct relation to the ductility of the test-bar taken across the section of the throw and near the centre of the original ingot. This is the point at which non-metallic enclosures are most likely to be found, as well as that at which the greatest stress occurs. The inference is obvious that a flaw is likely to develop at some sharp corner of such an enclosure. In a section of steel such as that which must be used non-metallic enclosures are very likely to occur, and so steps had to be taken to ascertain what the best practical remedy was. With decreased carbon content greater ductility was likely to follow, and this has been shown to be the case. In a word, it is toughness rather than strength which is required, and the studied consideration of these points has led to an increased life in miles of the crank axles of the 3,000 locomotives owned by the Company, in spite of the fact that they have been constantly growing in size, in pressure on the pistons, and in the work expected from them. This is shown in the following curves which represent the mileage of crank axles scrapped in the last twelve years



Average Mileage obtained from Crank Axles for Years 1910 to 1921 inclusive.

It will be appreciated that the above result, which is unquestionably the result of 'ordered knowledge of natural phenomena and the relation between them,' is only one example, if perhaps the most marked one, in our experience. A somewhat similar one could, however, be written on locomotive tyres and other matters if space and time permitted.

This example finishes my general remarks, and I cannot do so without expressing the indebtedness I feel to the various members of the scientific staff of our great firms for all the assistance and help they have ever so readily given us in the case I have just quoted.

One would like to press home strongly on engineers generally a point made by Dr. Maw in his Presidential Address to the Institution of Civil Engineers in November last. He pointed out the large amount of scientific knowledge—much of which was accumulated during the War—which is available at the present day. Here is the knowledge if we will but apply it to the service of man. This is our function as engineers. In times past we have had to wait for this knowledge, and as I trust I have shown, as it slowly became available it has been used in our service and in that of the world. One great need is for men with the education, the capacity, and the imagination necessary to use this scientific knowledge for the advancement of our profession. I use these three requisites advisedly, for each one of them is necessary to take full advantage of the opportunities which now exist. The trouble is that whereas we can supply education, can increase the capacity of the individual, it is difficult to instil or cultivate that imagination which allows one to see the way in which the knowledge available can be applied in a practical way.

I think I have shown adequately the debt which transport, as well as other branches of our profession, owes to the study of 'ordered knowledge.' That in the future this will be even more marked than at present, one can say without fear of contradiction. Not only so, but there must be more and more interdependence between science and engineering. More and more as we advance—as we are doing so rapidly—in the knowledge of natural phenomena will the necessity of the practical application of this knowledge on a large scale become necessary to confirm it and to bring out fresh features. One trusts that our Association, which has done so much in this direction in the past, may continue increasingly useful in the branch of its work which brings together those whose work is purely scientific with those who are applying that knowledge to the direct service of man. Although the old idea of antagonism between the two has disappeared, we cannot but feel that in spite of the advance of recent years the extent to which the engineer depends on the scientist for knowledge, and the scientist depends upon the engineer for the practical application of the knowledge he has gathered, is not realised as fully as it should be by either. The terms scientific and practical should be synonymous.

One would like to feel that the meeting of our Association was more generally used as the occasion on which the scientist and the engineer would meet in larger numbers. I know that the scientist is often an engineer, and that the engineer has nowadays to be a scientist with a broad outlook, but the personal contact of the two which this meeting offers gives an opportunity the results of which would be incalculable if that opportunity were fully grasped. If one might use an illustration which I trust will not offend my scientific friends, scientific knowledge is a tool of infinite possibilities, and this knowledge is possessed by so many who attend here. The practical engineer is always attracted by tools. There is no better method of ascertaining what new and improved tools of this type are available than by coming here. Beyond all this, personal acquaintance is of greater and more permanent value from every point of view than a paper acquaintance.

I would like, in closing, to make an appeal for a freer disclosure of results obtained in practical working. This can only be done by taking care in noting the behaviour of apparatus, material, &c., in use, and placing the results freely at the disposal of the man of science and of the manufacturers. At the present day there is no lack of those who are trained observers, and I believe one of the troubles often encountered by manufacturers who are applying some new method is the difficulty of getting dependable figures of performance. With transport companies this should not be a difficult matter, for one great advantage they have now is that there is no trade necessity to hide their results in any way. It is one small way in which they can repay the great debt they owe to science, which has allowed them to complete so satisfactorily their task. As Kipling has so rightly and concisely stated:

> ' It is their care that the wheels run truly, it is their care
> to embark and entrain,
> Tally, transport, and deliver duly the Sons of Mary by
> land and main.'

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SECTION H.—ANTHROPOLOGY.

EGYPT AS A FIELD FOR ANTHROPOLOGICAL RESEARCH.

ADDRESS BY

PROFESSOR P. E. NEWBERRY, M.A., O.B.E.,

PRESIDENT OF THE SECTION.

WHEN I received the honour of an invitation to preside at the Anthropological Section of the British Association my thoughts naturally turned to the subject of the Presidential Address, which, if I accepted the invitation, it would be my duty to prepare. On looking back over the Addresses of past Presidents of this Section since its institution in 1884 I found that no one had dealt with Egypt as a field for anthropological research. It was because of this that I decided to accept the Council's invitation, and I am here to-day to bring before your notice some facts regarding the civilisation of the country with which I have long been associated, and in which I have spent many years of my life.

In 1897, when the British Association last met in this great city on the Mersey-side, Sir Arthur Evans occupied the Presidential Chair of this Section, and the subject of his address was 'The Eastern Question in Anthropology.' Surveying the early history of civilisation as far as it was then known, he insisted that the adequate recognition of the Eastern background was essential to the right understanding of the Ægean. He laid stress on the part which Crete had played in the first emancipation of the European genius, and pointed out that in Crete, far earlier than elsewhere, can be traced the vestiges of primeval intercourse with the Nile Valley. Nineteen years later, years that were extraordinarily prolific in archæological discovery in every part of the Near East, Sir Arthur occupied the Presidential Chair of the British Association at Newcastle. He then addressed us on 'New Archæological Lights on the Origins of Civilisation in Europe.' Referring to his epoch-making discoveries in Crete he said, 'It is interesting to note that the first quickening impulse came to Crete from the Egyptian and not from the Oriental side; the Eastern factor in it is of comparatively late appearance.' By that time Sir Arthur's researches had led him to the 'definite conclusion that cultural influences were already reaching Crete from beyond the Libyan Sea, before the beginning of the Egyptian Dynasties.' He further said 'the impression of a very active agency indeed is so strong that the possibility of some actual immigration into the island of the older Egyptian element, due to the conquests of the first Pharaohs, cannot be excluded.'

I propose to-day to deal with some of the questions relating to the origins of the Egyptian civilisation, and incidentally shall touch upon

this Cretan problem. At the end of my address I shall very briefly refer to the much neglected modern Egyptians, and to the need there is to study them. Much has been written during the last twenty years about the origins of the Egyptian civilisation, but there are some facts which, I think, have either escaped notice or have not been duly considered, and there are others upon which, in my opinion, insufficient stress has been laid. I am not going to deal with the physical characteristics of the people, for that is not my province. I shall confine myself to certain inferences that I believe can be drawn from the monuments of pre-dynastic and dynastic times.

It is generally agreed that the habits, modes of life, and occupations of all communities are immediately dependent upon the features and products of the land in which they dwell. Any inquiry into Egyptian origins ought, therefore, to begin with the question, What were the physical conditions that prevailed in the Lower Nile Valley immediately preceding, and during, the rise of its civilisation? Until this question is answered I do not think that we are in a position to deal with such important problems as, *e.g.*—agriculture, architecture, shipbuilding, tool-making, or weaving. The first thing that we ought to know is what were the kinds of trees, plants, and animals that were to be found in Egypt in the wild state, and what was the economic value of the indigenous flora and fauna. We ought, in fact, to know what the country was like in pre-agricultural days. If there was no timber in the country, then it may, I think, be confidently said that the art of the carpenter did not originate in Egypt; that the architectural styles founded on wood construction could not have arisen there; that the art of shipbuilding (at all events of building ships of wood) did not originate there. Similarly, if there were no incense-bearing trees or shrubs in the country, it is difficult to imagine that the ceremonial use of incense arose there. Again, the art of weaving presupposes the presence of sheep or goats for wool, or of flax for linen thread. All these kinds of problems depend upon the natural products of a country, or they did so depend in the early days of civilisation.

We are accustomed to regard Egypt as a paradise, as the most fertile country in the world, where, if we but scratch the soil and scatter seed, we have only to await and gather the harvest. The Greeks spoke of Egypt as the most fit place for the first generations of men, for there, they said, food was always ready at hand, and it took no labour to secure an abundant supply. But there can be no doubt that the Egypt of to-day is a very different place from the Egypt of pre-agricultural times. There has been a great, but gradual, change in the physical condition of the whole country. In the mortuary chapels of tombs of the Old and Middle Kingdoms, as well as in many of the Empire, are scenes of papyrus swamps and reed marshes; in these swamps and marshes are figured the animals and birds that then frequented them. Among the animals are the hippopotamus and the wild boar, the crocodile, the ibis, and a great variety of water-fowl. These animals, and some of the birds, have now disappeared from the region north of the First Cataract. Only very recently has the crocodile become extinct north of Aswân. It was still occasionally seen in

the Delta as late as the middle of the eighteenth century, and it was fairly plentiful in Upper Egypt up to the middle of the nineteenth century, but it is now rarely, if ever, seen north of Wadi Halfa. It is the same with the hippopotamus. In the twelfth century this mammal still frequented the Damietta branch of the Nile, and two specimens were actually killed near Damietta by an Italian surgeon in the year 1600.[1] In the Dongola Province of Nubia it was very common at the beginning of last century, and Burckhardt states that it was then a terrible plague there on account of its voracity. In 1812 several hippopotami passed the Second Cataract and made their appearance at Wadi Halfa and Derr, while one was actually seen at Darâwi, a day's march north of Aswân.[2] The wild boar is apparently now extinct in Egypt, but specimens were shot in the Delta and in the region of the Wadi Natrûn during last century. The ibis has gradually disappeared from the Lower Nile Valley, where it was once so common. The last specimen of this bird recorded in Egypt was shot in 1877 in Lake Menzaleh. It is sometimes seen in Lower Nubia, but has now entirely disappeared from Egypt proper.

Much is known about the ancient fauna of the desert wadies from the paintings and sculptured scenes in the tombs of the Old and Middle Kingdoms and of the Empire. On the walls of many of these tombs are depicted hunting scenes,[3] and among the wild animals figured in them are the lion, leopard, Barbary sheep, wild ass, wild ox, hartebeest, oryx, ibex, addax, dorcas gazelle, fallow deer, giraffe, and ostrich. As several of these animals are not now known in Egypt it has been argued that the scenes do not faithfully represent the ancient fauna of the country. But I can see no reason to doubt that the scenes depict actual hunts that took place in the Arabian and Libyan Deserts not far from the localities in which the tombs figuring them are found. There is some corroborative evidence in the references in the ancient literature to the hunting of the wild animals that frequented Egypt. Thutmose IV., for example, hunted the lion and ibex in the desert plateau near Memphis;[4] Amenhotep III. killed 102 fierce lions during the first ten years of his reign,[5] and in his second regnal year he hunted wild cattle in the desert near Keneh;[5] he saw there a herd of 170, and of these he and his huntsmen captured 96. The desert to the east of Kûft was a famous hunting-ground at the time of the Eighteenth Dynasty. At the present day all but one of the animals represented in these ancient hunting scenes are found in the Nubian Deserts to the south of Egypt. The exception is important; it is the fallow deer which belongs to the Holarctic, not to the Ethiopian, zoological zone. Although most of the animals that were hunted by the dynastic Egyptians have now disappeared from their northern home, many have been recorded in recent years as occurring in the Arabian and Libyan Deserts. We can, in fact, follow them gradually receding southwards. The dorcas gazelle is still common in both deserts, and the addax sometimes occurs in the region of the Wadi Natrûn. The ibex is occasionally seen on the mountains north-east of Keneh. The Barbary sheep (*Ammotragus tragelaphus*) was observed by Dr. Schweinfurth in 1878 in the Wadi Shietun which opens on the Nile below Ekhmim.[6] The

wild ass was recorded by James Burton in 1823 in the desert north-east of Keneh; he remarks that then the Arabs of this part of the desert let their female donkeys loose to be served by the wild males.[7] Later, in 1828, Linant de Bellefonds saw many wild asses in the region between Darâwi and Berber; they were, he says, often trapped by the Bisharîn, who used the flesh as food. During the first half of the eighteenth century the ostrich frequented the desert near Suez.[8] A hundred years later it was reported to be numerous in the Arabian Desert opposite Esneh, and there is a wadi, some distance south-east of Aswân, that is called by the Arabs Wadi Naam, 'the Wadi of Ostriches.' In the Libyan Desert the bird was fairly common in the eighteenth century. W. G. Browne, who travelled along the coast west of Alexandria in 1792, states that tracks of the ostrich were frequently seen, and he noted also that the bird sometimes appeared in the neighbourhood of the Wadi Natrûn.[9] Geoffroy Saint-Hilaire in 1799 reported that it was then common in the mountains south-west of Alexandria.[10] In 1837 Lord Lindsay saw the ostrich near Esneh,[11] but the northern limit of the bird is now very much further south. The lion is mentioned by Sonnini at the end of the eighteenth century as one of the larger carnivora which then approached the confines of Egypt, but did not long remain in the country.

Now the appearance of all these animals in Egypt, and in its bordering deserts in dynastic times presupposes that the vegetation of the wadies was much more abundant then than now, and this again presupposes a greater rainfall than we find at present. The disappearance of the dynastic fauna is not, however, entirely due to the change in climatic conditions. The Arabs have a saying that it was the camel that drove the lion out of Egypt, and this is doubtless true. The lion depends mainly on the antelope tribe for its food supply. The antelopes, on the other hand, depend for their sustenance on herbage and grass, and this has been consumed to a great extent by the camels, which, since Arab times, have been bred in great numbers in the Arabian and Nubian Deserts. It is certain that the advent of the camel was a factor in driving southwards many of the wild animals that were at one time so common in Egypt, but are now characteristic of the Ethiopian region.

The characteristic wild trees of the dynastic flora of Egypt, as we know from the remains of them that have been found in the ancient tombs, were the heglik (*Balanites ægyptiaca*), the seyal (*Acacia seyal*), the sûnt (*Acacia nilotica*), the tamarisk (*Tamarix nilotica*), the nebak (*Zizyphus spina-Christi*), the sycomore-fig (*Ficus sycomorus*), and the moringa (*Moringa aptera*). The dom palm (*Hyphæne thebaica*) and the Dellach palm (*H. argun*) were also common. The heglik does not now grow wild north of Aswân, and of the other trees, only the sûnt and the tamarisk are really common in the Lower Nile Valley. All these trees, however, now grow in abundance in the region north of the Atbara, and it is here, in what is called the Taka country, that we find also the fauna that was once so abundant in more northerly regions.

But if the fauna and flora of the Arabian and Libyan Deserts in dynastic times approached more closely to that now seen in the Taka

country, we have to go further south again for the earliest pre-dynastic fauna and flora of the Lower Nile Valley. This pre-dynastic fauna is particularly interesting, because, in addition to several of the animals already mentioned as occurring in dynastic times, we meet with others, such as the elephant,[12] the kudu (*Strepceros kudu*),[13] the gerenuk gazelle (*Lithocranius walleri*),[14] a species of *Sus*[15] (which is certainly not the wild boar, *i.e. Sus scrofa*), and the marabou stork (*Leptoptilus crumenifer*).[15] From the nature and habits of these mammals and birds it is evident that there must have been a considerable rainfall in the Valley of the Nile north of Aswân when they frequented Egypt. Dr. Anderson has referred to this subject in his monograph on the Reptilia of Egypt. He notes that the physical features on both sides of the Nile 'indicate the existence of a period long antecedent to the present, in which a considerable rainfall prevailed, as in the eroded valleys of the desert may be observed rocky ravines which have been carved out by the action of water, which has left behind it dry channels over which waterfalls had once precipitated themselves, and others down which cataracts once raced. The rainfall of the present is not sufficient to account for such a degree of erosion.'[16] This evidence sanctions the conclusion that a material change in the character of the climate of North-Eastern Africa, so far as its rainfall is concerned, has taken place since pre-dynastic days. The flora of the valley of the Lower Nile also points to the same conclusion. Dr. Schweinfurth[17] has drawn attention to the fact that many plants, now known in Egypt only under cultivation, are found in the primeval swamps and forests of the White Nile. He not unreasonably draws the inference that in ages long ago the entire Nile Valley exhibited a vegetation harmonising in its character throughout much more than at present. The papyrus swamps and reed marshes that lined the Lower Nile Valley in pre-agricultural days have been changed into peaceful fields, in which now grow the cereal grains, wheat and barley, and the other crops that have made Egypt famous as an agricultural country. It was the canalisation of the Valley, carried out by man, and the consequent draining of the swamps and marshes that displaced the ancient flora from its northern seat, and made it, as at the present day, only to be found hundreds of miles higher up the river. The land of Egypt has, in fact, been drained by man; each foot of ground has been won by the sweat of his brow with difficulty from the swamp, until at last the wild plants and animals which once possessed it have been completely exterminated in it. The agricultural Egypt of modern times is as much a gift of man as it is of the Nile.

I have dwelt at some length on the ancient fauna and flora because I want to bring out as clearly as I can two facts concerning the Egypt of pre-agricultural days—the Egypt of the time before man began to win the alluvial soil for the purposes of agriculture. (1) The aspect of the Lower Nile must have been very different from what it is now; it was a continuous line of papyrus swamps and marshes inhabited by hippopotami, wild boars, crocodiles, and immense flocks of wild-fowl of all kinds; it was singularly destitute of trees or plants that could be put to any useful purpose, and timber-trees were non-existent; its

physical conditions resembled those prevailing on the banks of the White Nile to-day. (2) The deserts bordering the Lower Nile Valley on both sides were much more fertile, and their fauna and flora resembled that of the Taka country in Upper Nubia. Of the animals that frequented the wadies only the ass and the wild ox were capable of domestication. If man inhabited Egypt in pre-agricultural times—and there is no valid reason to suppose that he did not—he probably lived a wandering life, partly hunter, partly herdsman, in the fertile wadies that bordered the valley, only going down to the river to fish or to fowl or to hunt the hippopotamus. In the valley itself there was certainly no pasture-land for supporting herds of large or small cattle. It was probably also in these wadies that agriculture was first practised in Egypt. Even at the present day a considerable number of Ababdeh roam the wadies of the Arabian Desert between Keneh and the Red Sea, where, at certain seasons of the year, there is fair pasturage for small flocks of sheep and goats. I have myself seen many of these people in the course of several journeys that I have undertaken to the Red Sea coast. Some of these nomads sow a little barley and millet after a rain-storm, and then pitch their tents for a while till the grain grows, ripens, and can be gathered. They then move on again with their little flocks. What the Ababdeh do on a very small scale the Hadendoa of the Taka country do on a much greater one.

If we turn to the Taka country we see there people living under much the same physical conditions as those which must have prevailed in the Arabian and Libyan deserts in early times. The inhabitants of the Taka country are Hamite, and, as Professor Seligman has pointed out,[18] the least modified of these people are physically identical with the pre-dynastic Egyptians of Upper Egypt. I would suggest that they, like the fauna and flora of ancient Egypt, receded southwards under the pressure of the advance of civilisation, and that the physical conditions of the country have preserved them to a great extent in their primitive life and pursuits. The picture of the Taka as Burckhardt draws it would, I believe, describe almost equally well the earliest pre-dynastic Egyptians. This country, called El Gash by its inhabitants, has been described by Burckhardt.[19] In his day the people there were in the transition stage between the pastoral nomad and the agriculturist. It was a fertile and populous region. About the end of June large torrents coming from the south and south-west pour over the country, and in the space of a fortnight or so cover the whole surface with a sheet of water, varying in depth from two to three feet. These torrents were said to lose themselves in the eastern plain after inundating the country, but the waters remained upwards of a month in Taka, and on subsiding left a thick slime or mud upon the surface. Immediately after the inundation was imbibed the Bedawîn sowed their seed upon the mud, without any previous preparation whatever. The inundation was usually accompanied by heavy rains, which set in a short time before the inundation, and became most copious during its height. The rains lasted some weeks longer than the inundation; they were not incessant, but fell in heavy showers at short intervals. In

the winter and spring the people of Taka obtained their water from deep wells, extremely copious, dispersed all over the country, but at a considerable distance from each other. The people appeared to be ignorant of tillage; they had no regular fields, and the millet, their only grain, was sown among thorny trees. After the harvest was gathered the peasants returned to their pastoral occupations. When Burckhardt visited this region in the hottest part of the year, just before the period of the rains, the ground was quite parched up, and he saw but few cattle; the herds were sent to the Eastern Desert, where they fed in the mountains and fertile valleys, and where springs of water were found. After the inundation they were brought back to the plain. The quantity of cattle, Burckhardt believed, would have been greater than it was had it not been for the wild beasts which inhabited the district and destroyed great numbers of them. The most common of these wild animals were the lion and the leopard. The flocks of the encampment were driven in the evening into the area within the circle of tents, which were themselves surrounded by a thorny enclosure. Great numbers of asses were kept by all these Bedawîn. They also possessed many camels. The trees are described as being full of pigeons. The Hadendoa were the only inhabitants of Taka seen by Burckhardt. Each tribe had a couple of large villages built in the desert on the border of the cultivable soil, where some inhabitants were always to be found, and to which the population, excepting those who tended the cattle in the interior of the desert, repaired during the rainy season. After the waters had subsided they spread over the whole district, pitching their camps in those places where they hoped for the best pasturage, and moved about from month to month, until the sun parched up the herbage. The settlers in the villages meantime sowed the ground adjoining the neighbouring desert. The camps consisted of huts formed of mats; there were also a few huts with walls, resembling those in the countries of the Nile, but smaller. Even the settlers, however, preferred living in the open under sheds to inhabiting these close dwellings.

It has often been stated that civilisation in Egypt spread from the south, and considerable stress has been laid upon the fact that so many pre-dynastic and early dynastic remains have been found in Upper Egypt in the region between Edfu and Thinis, especially at Hierakonpolis and Naqada, and north of Naqada, in the neighbourhood of Abydos. Opposite Edfu is a desert route leading to the Red Sea; at Kûft, opposite Naqada, is the beginning of the road leading to Kosêr, the port on the Rea Sea. It has been thought that the people who brought culture to Egypt reached the Nile Valley by one or by both these routes from a 'God's Land' situated somewhere down the Red Sea coast. But throughout the whole history of Egypt culture has always come from the north, and spread southwards.

From a study of the monuments of the First Dynasty that had been found at Abydos and elsewhere in Upper Egypt I ventured, nearly twenty years ago,[20] to suggest the existence in pre-dynastic times of a Delta civilisation which, in culture, was far advanced beyond that of Upper Egypt, and I pointed out that it was probably to a Delta

civilisation that the Dynastic Egyptians owed their system of writing. I was led to this conclusion by the following facts. Although many pre-dynastic cemeteries had been thoroughly explored in Upper Egypt no grave had yielded a single fragment of hieroglyphic writing. The only inference that can be drawn from this is that hieroglyphic writing was unknown, or at all events unpractised, by the inhabitants of Upper Egypt before Dynastic times. On the other hand, the discoveries at Naqada, Hierakonpolis, and Abydos had shown us that all the essential features of the Egyptian system of writing were fully developed at the beginning of the First Dynasty. Hieroglyphic signs were already in full use as simple phonograms, and their employment as phonetic complements was well established. Determinative signs are found beginning to appear in these early writings, but, as Erman and Griffith have noticed, even as late as the Fifth Dynasty their use was very restricted in the monumental inscriptions, although they were common in the cursive and freely written texts of the Pyramids. At the very beginning of the First Dynasty the numerical system was complete up to millions, and the Egyptians had already worked out a solar year of 365 days. This was indeed a remarkable achievement.

These facts are of great significance, for it is clear that the hieroglyphic system of writing, as we find it at the beginning of the First Dynasty, must have been the growth of many antecedent ages, and yet no trace of the early stages of its evolution have been found on Upper Egyptian soil. There is no clear evidence, however, that the system was borrowed from any country outside Egypt; the fauna and flora of its characters give it every appearance of being indigenous. It is apparent, therefore, that we must seek the cradle of the Egyptian system of hieroglyphic writing elsewhere than in Upper Egypt, and as the fauna and flora of its characters are distinctly Egyptian the presumption is that it must be located to the Delta. An important indication as to the original home of Egyptian writing is given by the signs which, in historic times, were used to designate the points of the compass. The sign for 'east' was a drop-shaped ingot of metal upon a sacred perch, and this was the cult-object of a clan living in predynastic times in the Eastern Delta. The sign for 'west' was an ostrich feather placed in a semicircular stand, and this was the cult-object of the people of the Western Delta. The sign for 'south' was a *scirpus*-reed; this was the cult-object of a clan which dwelt on the east bank of the Nile a little above the modern village of Sharona in Middle Egypt. The country south of the apex of the Delta was known as *Ta Shema*, 'Reed Land.' It must, therefore, have been at some point north of the apex of the Delta that the scirpus-reed was first used to designate the south. It must also have been somewhere in the Central Delta that the cult-objects of the peoples of the Eastern and Western Delta were first used to designate 'east' and 'west.'

For the Delta being the early home of writing another fact has to be taken into consideration. Thoth, the Ibis-god, was to the Egyptians the god of writing, and it was to him that they attributed its invention. The principal seat of his worship in historic times was Hermopolis, in Middle Egypt. But Thoth's original habitat was

situated in the north-east corner of the Delta, where, in pre-dynastic times, had resided an Ibis clan. The tradition that named Thoth as the god and inventor of writing would, therefore, point Delta-wards. This tradition is significant also in another way. Although we cannot doubt that the Egyptian system of writing was evolved in the Delta, the germs of writing may have come into Egypt from Western Asia *via* this north-east corner of the country. In this connection it may be pointed out that the hieroglyphic signs for 'right' and 'left' were the same as those for 'west' and 'east'; the Egyptians who evolved the hieroglyphic system of writing orientated themselves facing south.

It is remarkable that so little is known about the early history of the Delta. But few excavations have been carried out there, and nothing of pre-dynastic, or early dynastic, times, has, so far, been brought to light from the country north of Cairo. We do know, however, that before the arrival of the Falcon-kings from Hierakonpolis in the south, Middle and Lower Egypt had been, probably for many centuries, united under one sceptre, and that before these two parts of the country were united there had been a Delta Kingdom which had had its capital at Sais. The names of some of these early kings are preserved on the Palermo fragment of the famous Annals Tablet, and the list there given would alone be enough to prove how ancient the Delta civilisation must have been. There was certainly nothing comparable with it in Upper Egypt in those far-off days.

What were the physical conditions prevailing in the Delta and in the regions to the east and west of it immediately preceding Menes' arrival in Lower Egypt? For the eastern side the evidence is exceedingly scanty, but there is one fact which is significant. The chief god of the eastern nomes of the Delta in the Pyramid Age was Anzety, a pastoral deity who was the prototype of Osiris. He is represented as a man holding in one hand the shepherd's crook, and in the other the goatherd's ladanisterion. There can be little doubt, therefore, that in the eastern Delta there lived a pastoral people who possessed flocks of sheep and goats, and this is evidence of a certain amount of grass-land. In the Central Delta at the same period there lived a series of clans, among which a Bull Clan was predominant. In historic times in Egypt the ox is often figured roaming in papyrus and reed marshes, and it may be that the Central Delta marshes supported herds of domesticated cattle. Much more is known about the western side of the Delta at the time of Menes. It formed, I believe, part of what was called Tehenu-land, at all events this name was given to the region immediately to the west of the Canopic branch of the Nile. There can be no doubt that this part of the country was a very fertile and prosperous region in the period immediately preceding the First Dynasty. Its name signifies 'Olive-land,' and we actually see these trees figured, with the name of the country beside them, on a pre-dynastic Slate Palette; on this Palette, above the trees, are shown oxen, asses, and sheep of the type later known as *ser*-sheep. It was Menes,[21] the Falcon-king of Upper Egypt, who conquered the people of Tehenu-land. This conquest is recorded on a small ivory cylinder that was found at Hierakonpolis. Another record of the Southerner's triumph over these

people is preserved on his famous Slate Palette; here the Upper Egyptian king is depicted smiting their Chieftain, while on the verso of the same Palette is the scene of a festival at the Great Port, which was perhaps situated near the Canopic branch of the Nile. The mace-head of Menes, which is now in the Ashmolean Museum at Oxford, has a scene carved upon it which shows the king assuming the Red Crown of Sais, and the inscription accompanying it records that he had captured 120,000 prisoners, 400,000 oxen, and 1,422,000 goats. This immense number of oxen and goats is clear evidence that the north-western Delta and the region to the west of it (Tehenu-land) must have included within its boundaries very extensive grass-lands. Several centuries after Menes, Sahure, a king of the Fifth Dynasty, captured in Tehenu-land 123,440 oxen, 233,400 asses, 232,413 goats, and 243,688 sheep. Senusret I. also captured in the same region 'cattle of all kinds without number.' This again shows how fertile the country must have been at the beginning of the Middle Kingdom. The history of this part of the Delta is most obscure. During the period that elapsed from the end of the Third Dynasty to the beginning of the Twenty-third, when Tefnakht appears upon the scene, we have hardly any information about it. What was happening at Sais and other great cities in the north-west of Egypt during the period from 2900 to 720 B.C.? There is an extraordinary lacuna in our knowledge of this part of the country. The people living there were certainly of Libyan descent, for even as late as the time of Herodotus the inhabitants deemed themselves Libyans, not Egyptians; and the Greek historian says that they did not even speak the Egyptian language. The pre-dynastic people who inhabited the greater part of the Lower Nile Valley were apparently of the same stock as these Libyans. There is a certain class of decorated pottery which has been found in pre-dynastic graves from Gizeh in the north to Kostamneh in the south. On this decorated pottery are figured boats with cult-objects raised on poles. Altogether some 170 vases of this type are known, and on them are 300 figures of boats with cult-signs. Of these, 124 give the 'Harpoon' ensign; 78 the 'Mountain' ensign; and 20 the 'Crossed Arrows' ensign. These cult-objects all survived into historic times; the 'Harpoon' was the cult-object of the people of the Mareotis Lake region; the 'Mountain' and 'Crossed Arrows' were the cult-objects of the people dwelling on the right bank of the Canopic branch of the Nile. Thus it will be seen that out of 300 boats figured on vases found in graves in the Lower Nile Valley south of Cairo, 222 belong to cults which can be located in the north-western corner of the Delta. Twenty-two boats bear the 'Tree' ensign, which was the early cult-object of the people of Herakleopolis, a city just south of the Fayûm. Ten bear the 'Thunderbolt' ensign of Ekhmim. The 'Falcon' on a curved perch appears on three boats, and this ensign undoubtedly represents the Falcon deity of Hierakonpolis. At the beginning of the historic period the cult-objects of the people of the north-western Delta included (1) the 'Harpoon,' (2) the figure-of-eight 'Shield with Crossed Arrows,' (3) the 'Mountain,' and probably (4) the Double Axe,[22] and (5) a Dove or Swallow.[22] With the exception of the 'Harpoon' all these cult-objects are also found in

Crete, a fact which is significant in view of Sir Arthur Evans' remark, quoted at the beginning of my address, to the effect that he considers the possibility of some actual immigration into the Island of the older Egyptian element due to the first Pharaohs. The 'Harpoon,' it should be noted, is the prototype of the bident, and later, of the trident of the Libyan god Poseidon.

Upon the mace-head of Menes the king is represented assuming the Crown of Neith of Sais. This is the earliest representation of the famous Sed Festival which is generally held to be a survival, in a much weakened form, of the ceremonial killing of the king, its essential feature being regarded as the identification of the king with the god Osiris. The festival was, I believe, of Libyan origin, and, at all events in its origin, it was not connected in any way with Osiris. On this mace-head the Upper Egyptian conqueror is shown seated under a canopy upon a daïs raised high above the ground. He is clad in a long, close-fitting garment; upon his head is the Red Crown of Sais, and in one of his hands is the so-called flail. Behind him is a group of officials, and upon either side of the daïs are two fan-bearers. In front of the king is a princess seated in a palanquin, and behind her are three men figured in the act of running. This is the earliest of a long series of representations of the festival, and we cannot doubt that the particular ceremony here depicted was the central one around which, in later times, the other ceremonies that we know were connected with it were grouped. There is no indication here of any ceremonial killing of the king, and the Red Crown which Menes wears is not characteristic of Osiris but of the goddess Neith of Saïs. In the Mortuary Temple of Neuserre at Abusir, in the Temple of Amenhotep III. at Soleb in Nubia, and in the Temple of Osorkon III. at Bubastis, the Sed Festival is represented in far greater detail, but still there is no indication of the ceremonial killing of the king, or of his identification with Osiris. These later scenes show that the festival was a great national one that was attended by all the great dignitaries of State, and by the priests of the gods from all the principal cities of Egypt. In these later representations the king's daughters and the running men play an important part. Inscriptions accompanying the scenes at Soleb[23] and Bubastis state that the king at this festival assumed the protection of Egypt and of the sacred women of the Temple of Amon. The Queen at these periods of Egyptian history was the High Priestess of Amon and the Head of the Harîm of the god. An important reference to the festival is found in the inscription of Piankhy. This Ethiopian king, in his triumphant march from Thebes towards the Delta, had captured Hermopolis, the capital of a petty king named Namlot (a Libyan Dynast), and when Piankhy made his entry into the city he was acclaimed by the people, who prayed that he would celebrate there a Sed festival. 'His Majesty proceeded to the palace of Namlot, and entered every chamber. He caused that there be brought to him the king's wives and the king's daughters. They saluted His Majesty in the fashion of women,' but the Ethiopian says that he would not turn his face to them, and he did not celebrate a Sed festival. The most important point in connection with the festival is that at it the

king assumed the protection of the land of Egypt. It was a kind of coronation festival On Menes' mace-head the king is shown assuming the Red Crown, while before him is the Princess of the country that he had conquered, and below her is a statement of the number of prisoners and cattle captured by him in her country.

Now what were the rules that regulated the succession to the kingship in Ancient Egypt? It is often assumed that the kingship was hereditary in the male line, and that the son regularly succeeded his father on the throne. But we know that many Egyptian kings were not the sons of their predecessors. We also know that at some periods, at all events, the sovereign based his claim to the kingship upon the fact that he had married the Hereditary Princess. Harmhab, at the beginning of the Nineteenth Dynasty, tells us that he proceeded to the palace at Thebes, and there, in the Great House (*pr-wr*), married the Hereditary Princess. Then the gods, 'the lords of the House of Flame (*pr-nsrt*), were in exultation because of his coronation, and they prayed Amon that he would grant to Harmhab the Sed festivals of Re.' It was after his marriage to the princess that Harmhab's titulary was fixed. The reference to the House of Flame is interesting because the kindling of fire was an important ceremony at the Sed Festival; it is figured at Soleb, and there a priestess called 'the Divine Mother of Suit' plays an important rôle. This priestess may be compared with Vesta, who always bore the official title of 'Mother,' never that of 'Virgin.' It is unnecessary for me to speak of the King's Fire and the Vestal Virgins whose duty it was to keep the perpetual fire burning; the material has been collected by Sir James Frazer. This ceremony of kindling fire suggests that the festival may have been a marriage festival, and the running men figured on the mace-head of Menes, and in later representations, also points to this interpretation of it. There can be little doubt that it was a Libyan festival; at all events it is first found when Menes assumed the Red Crown of Neith of Sais. When Menes had conquered the north-western Delta, he married the Hereditary Princess of the country. She was probably the eldest daughter, or perhaps the widow, of the Lower Egyptian king whose country he had seized. Marriage with the king's widow or eldest daughter carried the throne with it as a matter of right, and Menes' marriage, we can well believe, was a marriage of policy in order to clinch by a legal measure his claim to that crown which he had already won for himself in battle. Sir James Frazer has noted that sometimes apparently the right to the hand of the princess and to the throne has been determined by a race. The Libyan king Antæus placed his daughter Barce at the end of a race-course; her noble suitors, both Libyans and foreigners, ran to her as the goal, and the one who touched her first gained her in marriage. The Alitemnian Libyans awarded the Kingdom to the fleetest runner. According to tradition, the earliest games at Olympia were held by Endymion, who set his sons to run a race for the kingdom. In all the ceremonies connected with the Sed Festival I can see no feature that suggests the Osirification of the king. When he wears the Red Crown he assumes control of Lower Egypt; when he wears the White Crown he assumes control of Upper Egypt.

There is one further point connected with the western side of the Delta that must be noted. Glazeware (and glass) in Egyptian is called *tehent*; this was one of the chief articles of export of Tehenu-land. Just as we use the word 'china' for a kind of porcelain which first came to us from China, so the Egyptians called glass *thn.t* after the country of the north-western Delta from which they derived it. Here in this western side of Lower Egypt is an almost wholly unexplored field for the anthropologist.

I have already referred to the pastoral deity Anzety, who, in the Pyramid Age, was Chief of the nomes of the Eastern Delta. Among all the nome-gods he is the only one that is figured in human form; he stands erect holding in his right hand the shepherd's crook, and in his left the goatherd's ladanisterion. On his head is a bi-cornate object that is connected with goats, and on his chin is a false beard curled at the tip. He was not an oxherd, but a shepherd and goatherd. In later times the figure of this deity, in hieroglyphic writing, is regularly used as the determinative sign of the word *ity*, 'ruling prince,' 'sovereign,' a term that is only applied to the living king. In the Pyramid Texts, Anzety is entitled 'Head of the Eastern nomes,' and these included the ancient one of the Oxyrrhynchus-fish, where, later, the ram or goat was the chief cult-animal. Neither the domesticated sheep nor the goat can be reckoned as Egyptian in origin; they both came into Egypt from Western Asia. We have, therefore, in this pastoral deity Anzety evidence of immigration from the west. The only wild sheep inhabiting the continent of Africa is the Barbary sheep, and this animal was not the ancestor of any domesticated breed. Both the sheep and the goat are essentially mountain animals, though sheep in the wild state do not as a rule frequent such rugged and precipitous ground as their near relatives the goats, but prefer more open country. Sheep browse in short grass; goats feed upon the young shoots of shrubs and trees. The domesticated goat is generally recognised as descended from the wild goat (*Capra hircus ægagrus*) of Syria, Asia Minor, Persia, and the Mediterranean Isles. Two breeds of domesticated sheep were known to the Egyptians. The sheep of the earliest historical period down to the Middle Kingdom was a long-legged variety (*Ovis longipes*), with horns projecting transversely and twisted. This breed was the only one known in the earlier periods of Egyptian history; it was the predominant breed in the Middle Kingdom, but soon after the beginning of the Empire it appears to have become rare or extinct in Egypt, and was superseded by a variety with horns curving forwards in a sub-circular coil. Both varieties of domesticated sheep, according to Lydekker, were introduced into Egypt through Syria.

Among the cult-objects of the cities over which the god Anzety presided were two, which, I believe, can definitely be referred to trees that were not indigenous to the soil of Egypt but to Syria. One of these cult-objects is the so-called Ded-column. This was one of the holiest symbols of the Egyptian religion. It has four cross-bars at the top like superposed capitals. Sometimes a pair of human eyes are shown upon it, and the pillar is draped: sometimes a human form is given to it by carving a grotesque face on it, robing the lower part,

crowning the top with ram's horns, and adding two arms, the hands holding the crook and ladanisterion. Frazer has suggested that this object might very well be a conventional representation of a tree stripped of its leaves. That it was, in fact, a lopped tree is, I believe, certain. In the Pyramid Texts it is said of Osiris, 'Thou receivest thy two oars, the one of juniper (*uan*), the other of *sd*-wood, and thou ferriest over the Great Green Sea.' The determinative-sign of the word *sd* is a tree of precisely the same form as the Ded-column that is figured on early Egyptian monuments, *i.e.* it has a long, thin stem. This tree-name only occurs in inscriptions of the Pyramid Age, and it is mentioned as a wood that was used for making chairs, tables, boxes, and various other articles of furniture. In the passage quoted from the Pyramid Texts it is mentioned together with juniper, and the latter was employed in cabinet-making, etc., at all periods of Egyptian history. There is no evidence that juniper ever grew in Egypt, but we have numerous records of the wood being imported from the Lebanon region. The *sd*-tree, as we see from the determinative-sign of the name, had horizontally spreading branches, and was evidently some species of conifer. No conifers, however, are known from Egypt; the *sd*-wood must, therefore have been of foreign importation. As it is mentioned with juniper, which we know came to Egypt from Syria, it is possible that it came from the same region. Among the trees of the Lebanon there are four that have horizontally spreading branches. These are the cedar (*Cedrus libani*), the Cilician fir, the *Pinus laricio*, and the horizontal-branched cypress (*Cupressus sempervirens* var. *horizontales*). Much misconception at present exists with regard to the Lebanon Cedar, because the name 'cedar' is applied to a large number of woods which are quite distinct from it, and the wood which we generally call cedar (*e.g.* the cedar of our 'cedar' pencils) is not true cedar at all, but Virginian juniper. The wood of *Cedrus libani* is light and spongy, of a reddish-white colour, very apt to shrink and warp badly, by no means durable, and in no sense is it valuable. Sir Joseph Hooker, who visited the Lebanon in 1860, notes that the lower slopes of that mountain region bordering the sea were covered with magnificent forests of pine, juniper, and cypress, 'so that there was little inducement for the timber hewers of ancient times to ascend 6,000 feet through twenty miles of a rocky mountain valley to obtain cedar wood which had no particular quality to recommend it. The cypress, pine, and tall, fragrant juniper of the Lebanon, with its fine red heart-wood, would have been far more prized on every account than the cedar.' The *sd*-tree was, I believe, the horizontal-branched cypress which is common in the wild state. In the Middle Ages this tree was believed to be the male tree, while the tapering conical-shaped cypress was considered to be the female. This is an interesting fact, because there is some evidence to show that the tapering variety was the symbol of Hathor-Isis, while the horizontal-branched one was the symbol of Osiris.

In the Pyramid Age there are several records of the priests of the Ded-column. They were called 'priests of the venerable ded-column.' The seat of the cult was Dedu, or, as it was sometimes called, *Pr-Wsr*, 'the House of Osiris,' the Greek Busiris in the Central Delta. At this

city was celebrated annually a great festival in honour of Osiris. It lasted many days, and the culmination of a long series of ceremonies was the raising of the ded-column into an erect position. Osiris is intimately connected with this column; the Egyptians called it his backbone. In the myth of Osiris, as recorded by Plutarch, a pillar played an important part. Plutarch says that the coffer containing the body of Osiris was washed up by the sea at Byblos, the port of the Lebanon, and that a tree grew up and concealed the coffin within itself. This sacred tree was cut down by Isis and presented to the people of Byblos wrapped in a linen cloth, and anointed with myrrh like a corpse. It therefore represented the dead god, and this dead god was Osiris.

Not far from Dedu, the city of Osiris in the Delta, was Hebyt, the modern Behbeyt el Hagar. Its sacred name was Neter. The Romans called it Iseum, or Isidis oppidum. It was the ancient seat of Isis worship in Egypt, and the ruins of its temple to that goddess still cover several acres of ground in the neighbourhood. On the analogy of other sacred names of cities the primitive cult-object here was the *ntr*-pole. This was not an axe as has so often been supposed, but a pole that was wrapped around with a band of coloured cloth, tied with cord half-way up the stem, with the upper part of the band projecting as a flap at top. Dr. Griffith conjectured that it was a fetish, *e.g.* a bone carefully wound round with cloth, but he noted that 'this idea is not as yet supported by any ascertained facts.' As a hieroglyh this wrapped-up pole expresses *ntr*, 'god,' 'divine,' in which sense it is very common from the earliest times; gradually it became determinative of divinity and of the divine names and ideographic of divinity. Another common ideograph of 'god' in the Old Kingdom was the Falcon (Horus) upon a perch, and this sign was also employed as a determinative of divinity and of the names of individual gods; it even sometimes occurs as a determinative sign of the *ntr*-pole, *e.g.* Pyr. Texts, 482. This use of the Falcon indicates that in the early dynasties the influence of the Upper Egyptian Falcon-god (Horus) was paramount. But there is reason for believing that the *ntr*-pole cult had at an earlier period been the predominant one among the writing people of the Delta; this, I think, is shown by the invariable use of the *ntr*-pole sign in the words for priest (*hm-ntr*, 'god's servant'), and temple (*ḥt-ntr*, 'god's house'). Now, on a label of King Aha of the First Dynasty there is a representation of the temple of Neith of Sais. Here two poles with triangular flags at top are shown on either side of the entrance. Later figures of the same temple show these poles with the rectangular flags precisely as we find in the *ntr*-sign. A figure of the temple of Hershef on the Palermo Stone shows two poles with triangular flags, while a Fourth Dynasty drawing of the same temple shows the same poles with rectangular flags. We see, therefore, that the triangular-flagged pole equals the rectangular-flagged one, and that the *ntr* is really a pole or mast with flag. Poles of this kind were probably planted before the entrances to most early Egyptian temples, and the great flag-masts set up before the pylons of the great temples of the Eighteenth and later dynasties are obviously survivals of the earlier poles. The height and straightness of these poles prove that they cannot have been produced

by any native Egyptian tree; in the Empire flag-staves were regularly imported from Syria; it is probable therefore that in the earlier times they were introduced from the same source. A well-known name for Syria and the east coast of the Red Sea, as well as of Punt, was Ta-ntr, 'the land of the *ntr*-pole.' This was the region in which the primitive Semitic goddess Astarte was worshipped. In Canaan there was a goddess Ashera whose idol or symbol was the ashera pole. The names of Baal and Ashera are sometimes coupled precisely as those of Baal and Astarte, and many scholars have inferred that Ashera was only another name of the great Semitic goddess Astarte. The ashera-pole was an object of worship, for the prophets put it on the same line with the sacred symbols, such as Baal pillars; the ashera was, therefore, a sacred symbol, the seat of a deity, the mark of a divine presence. In late times these asherim did not exclusively belong to any one deity; they were erected to Baal as well as to Yahw. They were sign-posts set up to mark sacred places, and they were, moreover, draped. They correspond exactly to the *ntr*-poles of Egyptian historic times. I have noted that these *ntr*-poles were tall and straight. What tree produced them? In Egyptian inscriptions there is often mentioned a tree named *tr.t*. It was occasionally planted in ancient Egyptian gardens, and specimens of it were to be seen in the Temple garden at Heliopolis. The seeds and sawdust were employed in medicine, and its resin was one of the ingredients of the Kyphi-incense. Chaplets were made of its twigs and leaves. The tree was sacred to Hathor; branches of it were offered by the Egyptian kings to that goddess. In a Saite text it is mentioned with three other trees—pine, yew, and juniper; these are all found in Northern Syria, where they grow together with the cypress; the *tr.t* tree may therefore be the cypress. Evidence has been brought forward to show that the *sd*-tree is the horizontal-branched cypress, which was believed to be a male tree, while the tapering, flame-shaped cypress was believed to be the female tree. The ded-column was the symbol of Osiris, and at Busiris was celebrated a festival of raising this column. The *tr.t* tree was sacred to Hathor, who is often identified with Isis, and there was a festival of raising the *tr.t* tree that was celebrated on the nineteenth day of the first month of the winter season. It is not known where this festival was celebrated, but it may well have been at Neter, the seat of the Isis cult near Dedu-Busiris. The two tree-cults point to Northern Syria as the country of their origin.

In the architecture of ancient Egypt two distinct styles can be recognised. One is founded on wattle-and-daub, the other on wood construction. Wattle-and-daub is the natural building material of the Nile Valley and Delta, and the architectural forms derived from it are certainly indigenous. Those styles derived from wood construction, on the other hand, could not have originated in Egypt, but must have arisen in a country where the necessary timber was ready at hand. Egypt produces no coniferous trees and no timber that is at all suitable for building purposes, or indeed for carpenter's work of any description. The wood of the sycomore-fig is very coarse-grained, and no straight planks can be cut from it. The sûnt-acacia is so hard that it requires to be sawn while it is green; it is very irregular in texture,

and on account of the numerous branches of the trunk it is impossible to cut it into boards more than a couple of feet in length. The palaces of the early kings of the Delta were built of coniferous wood hung with tapestry-woven mats. The tomb of Menes' queen, Neith-hotep, at Naqada, was built of brick in imitation of one of these timber-constructed palaces, and smaller tombs of the same kind are known from the Second and Third Dynasties, but not later. As early as the reign of King Den (First Dyn.) the palaces of this type were beginning to be built of the native wattle-and-daub in combination with wood, and by the end of the Pyramid Age the style disappears entirely, though the memory of it was preserved in the false-doors of the tombs and stelæ. Brick buildings similar to those of the 'palace' style of Egypt are also known from early Babylonia, and they were at one time regarded as peculiarly characteristic of Sumerian architecture. These, obviously, must have been copied, like the Egyptian, from earlier timber forms. In Babylonia, as in Egypt, timber was scarce, and there are records that it was sometimes obtained from the coast of Syria. This was the region from which the Egyptians throughout historic times obtained their main supplies of wood, so it is not improbable that they, as well as the Sumerians, derived this particular style of architecture from Northern Syria. I may observe in passing that in this 'palace' style we have the transition form between the nomad's tent and the permanent building of a settled people. The lack of native timber in Egypt is significant in another direction. Boats of considerable size are figured on many pre-dynastic monuments. They are long and narrow, and in the middle there is usually figured a reed or wicker-work cabin. In my view these boats were built, like many of those of later periods in Egypt, of bundles of papyrus reeds bound together with cord; they were, in fact, great canoes, and, of course, were only for river traffic. They were not sailing boats, but were propelled by means of oars. No mast is ever figured with them, but they generally have a short pole amidships which is surmounted by a cult-object. On one pre-dynastic vase there is a figure of a sailing ship, but this is totally different in build from the canoes, and it has a very high bow and stern with its mast set far forward in the hull. Similar vessels are figured on the ivory knife-handle of pre-dynastic date from Gebel el Araq, but these vessels appear to be in port and the sails are evidently lowered. I have already referred to the Great Port mentioned on the Palette of Menes. A port implies shipping and trade relations with people dwelling along the coast or across the sea. It may be that the people of the north-western Delta built wooden ships, but if they did they must have procured their timber from some foreign source. Coniferous wood was already being imported into the Nile Valley at the beginning of the First Dynasty from the Lebanon region, and it must be remembered that the Egyptian name for a sea-going ship was *kbnyt*, from *Keben*, 'Byblos,' the port of the Lebanon, where these ships must have been built and from whence they sailed. The sacred barks of the principal gods of Egypt in historic times were invariably built of coniferous wood from the Lebanon. Transport ships on the Nile were sometimes built of the native sûnt-wood, and Herodotus describes them as made of planks about two cubits

long which were put together 'brick-fashion.' No masts or sail-yards, however, could possibly be cut from any native Egyptian tree. In the Sûdan at the present day masts are sometimes made by splicing together a number of small pieces of sûnt and binding them with ox-hide, but such masts are extremely liable to start in any gale, and they would be useless for sea-going ships. It may be doubted whether the art of building sea-going ships originated in Egypt. It may be doubted also whether the custom of burying the dead in wooden coffins originated in Egypt. In countries where a tree is a rarity a plank for a coffin is generally unknown. In the Admonitions of an Egyptian Sage written some time before 2000 B.C., at a period when there was internal strife in Egypt, the Sage laments that 'Men do not sail northwards to [Byb]-los* to-day. What shall we do for coniferous trees† for our mummies, with the produce of which priests are buried, and with the oil of which [chiefs] are embalmed as far as Keftiu? They come no more.' This ancient Sage raises another anthropological question when he refers to the oil used for embalming. The only oils produced by native trees or shrubs in Egypt were olive oil, ben oil from the moringa, and castor oil from the castor-oil plant. The resins and oils used for embalming were principally those derived from pines and other coniferous trees. Egypt produced no kinds of incense trees or shrubs. The common incenses were pine resin, ladanum, and myrrh, and all these were imported. It is difficult to believe that the ceremonial use of incense arose in Egypt.

These are a few of the questions raised by a study of the material relating to the origins of the ancient civilisation of Egypt. There are numbers of others that are waiting to be dealt with. Egypt is extraordinarily rich in material for the anthropologist. It is a storehouse full of the remains of man's industry from pre-agricultural times right down to the present day. Almost every foot of ground hides some relic of bygone man. The climatic conditions prevailing there are exceptional, and it is largely owing to the absence of rain that so full a record of man and his works has been preserved. For more than a century excavators have been busy in many parts of the country, but there is yet no sign that the soil is becoming exhausted; it is, in fact, almost daily yielding up its buried treasures. The past two or three decades have been prolific in surprises. Mines of hidden wealth have been unearthed where but a few years ago we only saw the sands and rocky defiles of the desert. Since we met at Hull last year, the most sensational archæological discovery of modern times has been made in a place that had been abandoned by many excavators as exhausted. This discovery, due to the untiring persistence of an Englishman, promises to yield results of extraordinary interest, but it will take years before they can be adequately published. Other discoveries have been made in Egypt during recent years which have opened out a vista of human history that we little dreamt of a quarter of a century ago. Three decades

* This place-name ends *-ny*: the restoration [*Kp-*]*ny* is due to Sethe and 'suits the traces, the space and context quite admirably.'—A. H. Gardiner, *The Admonitions of an Egyptian Sage*, Leipzig, 1909, p. 33.

† The word is *as*, a generic one for pines, fir, &c.

ago not a single monument was known that could be ascribed with certainty to the period before the Third Egyptian Dynasty. To-day we possess a continuous series of written documents which carry us back to Menes, the Founder of the Monarchy, some 3,400 years or more before our era. These written documents, moreover, show clearly that Menes himself must have come at the end of a very long period of development. Egypt had already had a long history when the Upper and Lower Countries were first united under a single sceptre. From Upper Egypt we possess a continuous series of uninscribed monuments which take us back far into prehistoric times. An immense vista has been opened out before our eyes by the discoveries of the last thirty years, and now, in Egypt better than in any other country in the world, we can see man passing from the primitive hunter to the pastoral nomad, from the pastoral nomad to the agriculturalist, and then on to the civilised life which begins with the art of writing. We can see in the Delta and in the Lower Nile Valley tribes becoming permanently settled in fixed abodes around primitive cult-centres, and then uniting with others into one community. We can trace the fusion of several communities into single States, and then, later, the uniting of States under a supreme sovereign. What other country in the world preserves such a record of its early history?

I have but little time left to speak of the modern Egyptians, but to the anthropologist few people are more interesting. In almost every circumstance of daily life we see the Old in the New. Most of the ceremonies from birth to burial are not Muslim, or Christian, or Roman, or Greek; they are Ancient Egyptian. In the transition of a people from one religion to another the important institutions of the older doctrine are generally completely abolished; many ceremonies and much unessential detail, however, survive, and in the Delta and Lower Nile Valley survivals are extraordinarily numerous. It was Lady Duff Gordon who said that Egypt is a palimpsest in which the Bible is written over Herodotus, and the Koran over that; the ancient writing is still legible through all. There is a passage in one of her letters which describes her visit to some Nubian women. Their dress and ornaments were the same as those represented in the ancient tomb-paintings. Their hair was arranged in little plaits, finished off with lumps of yellow clay burnished like golden tags. In their house, Lady Duff Gordon sat on a couch of ancient Egyptian design, with a semicircular head-rest, They brought her dates in a basket such as you may see in the British Museum. So closely did they and their surroundings resemble the scenes of the ancient tombs that she says she felt inclined to ask them how many thousand years old they were! The modern worship of the people is full of the ancient; many of the sacred animals and trees have taken service with Muslim Saints. Up to a few years ago cats were still fed by the 'Servant of Cats' in the Kadi's court in Cairo. Cobras are still held in great reverence in the City of the Khalifs. Some time ago the Director of the Zoological Gardens in Cairo told me that it was most difficult to procure cobras for the Gardens. It was not because they were scarce, but because the demand for them was so great that the price asked was far more than the Government would pay.

Many cobras, I was told, were kept in the upper rooms of houses in the native quarters of the city. The funeral customs of the people throughout the country are much the same as those which prevailed in ancient times. It is not only among the merchant and agricultural classes that we find the Old in the New. Mrs. Poole, the sister of the Arabic scholar Edward Lane, writing from Cairo in 1846, describes the scenes in one of Mohammed Ali's palaces on the death of a princess of the Royal Family. Immediately the royal lady breathed her last, her relations and slaves broke up all the beautiful china and glass which had been her property. 'The destruction after a death,' Mrs. Poole remarks, 'is generally proportioned to the possessions of the deceased; therefore, in this case, it was very extensive.' Many, perhaps most, of the festivals of the country are of ancient origin. In the Delta towns and villages there are several which are similar to those that were held there in ancient days. It is the same in Upper Egypt. Thebes still possesses its sacred boat, and on the festival commemorating the birthday of Luxor's patron saint, Abu'l Haggag, this lineal descendant of the sacred bark of Amon decorated with flags and gaily coloured bits of cloth, is drawn around the town in procession, amid the acclamations of the people. Modern Egypt has hardly been touched by the anthropologist. The Government official usually holds himself far too aloof to ever really get into intimate contact with the native. Edward Lane did much to record the manners and customs of the Cairene Egyptian, but he never lived among the fellahîn, and his book contains little about the modern dweller on the banks of the Nile outside Cairo. A rich harvest awaits any student who, knowing the language, will settle and live throughout the year among the peasants in any village or town in the Lower Nile Valley or Delta. It is only in this way that a real knowledge of the people can be obtained. Far less is known about them than about many a tribe in Central Africa.

Thucydides, in the preface to his 'History,' proposed to record past facts as a basis of rational provision in regard to the future, but he was not the first to whom this great thought had occurred. A thousand years before the Greek historian was born an old Vizier of Egypt said of himself that he was 'skilled in the ways of the Past,' and that 'the things of Yesterday' caused him 'to know To-morrow.' Anthropology, the Science of Man and Civilisation, aims at discovering the general laws which have governed human history in the past and may be expected to regulate it in the future. The Egyptian Vizier had, at most, a couple of thousand years of recorded history before him. Since his time the area of history has been ever widening, and we ourselves can look back over nearly six thousand years of human endeavour. We know considerably more of the past than did our forefathers, and though those who hold the reins of government do not usually learn by experience, the anthropologist ought to be able to predict a little better than the politician about the future. For thousands of years Egypt has been under foreign rule. It has been under the yoke of Ethiopian and Persian kings, under the Greek and Roman, Arab and Ottoman conquerors. Its people suffered three thousand years of oppression. For the last forty years it has had English justice. Egypt has this year

been handed back to the Egyptians. It is an Oriental country. What will be the immediate future of its people? It is not difficult to predict. Seventy years ago, when Egypt was under the sway of Said Pasha, there was current among the fellahîn of Thebes a little parable, and with this I will conclude. I quote it as it was taken down by Rhind in the fifties of last century, but the story was still remembered when I lived among the natives of Upper Egypt twenty-eight years ago. It runs thus:—

'It happened once that a Sultan captured a lion, which it pleased him to keep for his royal pleasure. An officer was appointed especially to have in charge the well-being of the beast, for whose sustenance the command of His Highness allotted the daily allowance of six pounds of meat. It instantly occurred to the keeper that no one would be a bit the wiser were he to feed his dumb ward with four pounds, and dispose of the remaining two for his own benefit. This he did, until the lion gradually lost his sleekness and vigour, so as to attract the attention of his Royal Master. "There must be something wrong," said he; "I shall appoint a superior officer to make sure that the former faithfully does his duty." No sooner was the plan adopted than the first goes to his new overseer, and convincing him very readily, that if the proceeds of two pounds be conveyed to their pockets, the meat would be far better employed than in feeding the lion, they agreed to keep their own counsel and share the profit between them. But the thirst of the newcomer soon becomes pleasantly excited by the sweets of peculation. He talks the matter over with his subordinate, and they have no difficulty in discovering that the lion might very well be reduced to three pounds a day. Drooping and emaciated, the poor beast pines in his cage, and the Sultan is more perplexed than before. "A third official shall be ordered," he declares, "to inspect the other two"; and so it was. But they only wait for his first visit to demonstrate to him the folly of throwing away the whole six pounds of meat upon the lion, when with so little trouble they could retain three, one apiece, for themselves. In turn his appetite is quickened and he sees no reason why four pounds should not be abstracted from his ward's allowance. The brute, he states to his colleagues, can do very well on two, and if not, he can speak to nobody in complaint, so why need they lose the gain? And thus the lion, reduced to starvation-point, languishes on, robbed and preyed upon by the overseers set to care for him, whose multiplication has but added to his miseries.'

NOTES.

(1) Buffon's *Hist. Nat.*, vol. xii., 1764, p. 24.
(2) Burckhardt, *Travels in Nubia*, 1819, p. 67.
(3) For a characteristic hunting scene of the Pyramid Age see Borchardt, *Grabdenkmal des Königs Sahure*; for one of the Middle Kingdom, Newberry, *El Bersheh* I, pl. vii.
(4) The Sphinx Stela, 1, 5.
(5) Newberry, *Scarabs*, pls. xxxiii.-iv.
(6) *Giornale l'Esploratore*, anno ii., fasc. 4.
(7) Brit. Mus., Add. MS., 25666.
(8) Burckhardt, *Travels in Syria*, 1822, p. 461.
(9) W. G. Browne, *Travels in Africa, &c.*
(10) *Mém. sur l'Egypte*, vol. i., p. 79.
(11) *Letters on Egypt, &c.*, ed. 1866, p. 107.
(12) *Journal of Egyptian Archæology*, vol. v., p. 234, pl. xxxiii.
(13) Petrie, *Abydos I*, pl. L.
(14) Lydekker, Brit. Mus., *Guide to the Great Game Animals*, 1913, p. 39, and figs. 21, 22.
(15) *Journal of Egyptian Archæology*, vol. v., pl. xxxiii., p. 227.
(16) Anderson, *Zoology of Egypt* (Reptilia), p. xlvi.
(17) Schweinfurth, *Heart of Africa*, vol. i., p. 69.
(18) C. G. Seligman, *Journal of the Anthropological Institute*, vol. xliii., p. 595.
(19) Burckhardt, *Travels in Nubia*, p. 387, *et seq.*
(20) *Proceedings* of the Society of Biblical Archæology, Feb. 1906, p. 69.
(21) That Narmer was Menes is proved by a sealing published by Petrie in *Royal Tombs of the Earliest Dynasties*, pl. xiii., 93. His conquest of Tehenu-land is recorded on an ivory cylinder published by Quibell, *Hierakonpolis I*, pl. xv., 7.
(22) The cults of the Double Axe and of the Dove or Swallow are found on monuments of the Pyramid Age.
(23) I owe my knowledge of the greater part of the Soleb scenes to Prof. Breasted, who kindly showed me unpublished drawings of them when I visited him in Chicago in 1921.

SECTION I.—PHYSIOLOGY.

SYMBIOSIS IN ANIMALS AND PLANTS.

ADDRESS BY

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CONTENTS.

Introduction.

THE subject of symbiosis has been chosen for this address because of its broad biological interest, an interest that appeals equally to the physiologist, pathologist, and parasitologist. It is, moreover, a subject upon which much work has been done of recent years in different countries, and this seems a fitting occasion upon which to give a brief summary of what is known to-day, especially since the literature relating to symbiosis is largely foreign, somewhat scattered and relatively inaccessible.

I. Symbiosis in Plants.

(1) *Lichens.*

It is well known to botanists that the vegetative body (thallus) of lichen plants consists of two distinct organisms, a fungus and an alga. The alga, individual elements of which are called 'gonidia,' is either scattered throughout the thallus or, as in most cases, it forms a well-defined layer beneath the surface of the thallus. The view that lichens

consist of the two elements mentioned was advanced by Schwendener (1867-9), who regarded the fungus as living parasitically upon the alga, a view which gained support from the researches of Bornet (1872), Voronin (1872), Treube (1873), etc., and especially of Bonnier (1886-9), wherein synthetic cultures were obtained by bringing together (*a*) various algæ obtained in the open and (*b*) fungus-spores isolated from cultures of fungi forming the one component of certain lichens.

Schwendener's view, that the fungi are parasitic on the algæ in lichens, was contested by Reinke (1873) on the ground that a state of parasitism did not explain the long and apparently healthy life of the associated fungi and algæ, a biological association for which the term *Consortium* was proposed by him, that of *Homobium* by Frank (1876), and that of *Symbiosis* by de Bary (1879), the latter term denoting *a condition of conjoint life that is more or less beneficial to the associated organisms or symbionts.*

Investigation has shown that the relation or balance between the associated organisms varies in different lichens. Thus in some forms of *Collemaceæ*, as stated by Bornet (1873), the partners as a rule inflict no injury upon each other, whilst in some species of *Collema* occasional parasitism of the fungus upon the alga (*Nostoc*) is observable, since short hyphal branches fix themselves to the alga cells, these swelling, their protoplasm becoming granular and finally being voided. In *Synalissa* and some other lichens the hypha penetrates into the interior of the alga, where it swells and forms a sucker, or haustorium. Elenkin (1902-6) and Danilov (1910) take it as proved that lichens owe their origin to parasitism, the fungus either preying upon the alga or living as an 'endosaprophyte' (Elenkin) upon the algæ that die.

Therefore, we may find in lichens the condition of true symbiosis on the one hand, ranging to demonstrable parasitism on the other, and, conversely to what has been described above, instances are known wherein algæ are parasitic on fungi (Beijerinck, 1890).

Physiology of Lichens.

The nutrition of algæ in lichens is similar to that of other chlorophyllaceous plants, the most important work on the subject being that associated with the names of Beijerinck (1890) and Artari (1902). In respect to nitrogen supply, Beijerinck cultivated various green algæ, as well as gonidia derived from *Physcia parietina*. The gonidia only multiplied rapidly in a malt-extract culture-medium to which peptones and sugar were added. This showed that the algæ associated with fungi as in lichens were placed advantageously in respect to nitrogen supply. He termed such fungi 'ammonia-sugar-fungi,' because they extract nitrogen from ammonia salts and, in addition to sugar, form peptones. Artari showed that there exist two physiological races in green algæ, those which absorb and those which do not absorb peptones. He found that the gonidia (*Cystococcus humicola*) derived from *Physcia parietina* absorbed peptones, and he consequently referred to such algæ as 'peptone-algæ.' Treboux (1912), however, denies the existence of peptone-sugar-races of algæ, and regards the algæ in lichens as the victims of parasitic fungi. Nevertheless, the important researches of

Chodat (1913) have demonstrated that cultivated gonidia develop four times as well when supplied with glycocoll or peptone in place of potassium nitrate.

The carbon supply of gonidia, according to Artari (1899, 1901), Radais (1900), and Dufrenoy (1918), is not derived photosynthetically, but from the substratum on which they grow. Whilst Tobler (1911), in his culture experiments with lichens, found that the gonidia obtain their carbon from calcium oxalate secreted by the fungus, Chodat (1913) observed that cultured gonidia grow but slowly without sugar (glucose), which he believes constitutes their main source of carbon supply.

Whereas, according to Chodat, the gonidia grow poorly on organic nitrogen in the absence of sugar, they develop rapidly when sugar is added. He therefore concludes that the gonidia lead a more or less saprophytic life in that they obtain from the fungus-hyphæ both organic nitrogen and carbon in the form of glucose or galactose.

The nutrition of fungi in lichens depends partly upon parasitism, when they invade the gonidia, and partly upon saprophytism, when they utilise dead gonidia (Chodat).

In concluding this section, the hypothesis of M. and Mme. Moreau (1921) demands mention, since it bears upon the manner in which lichens may have originated in nature. They regard the fungal portion as a gall-structure arising from the action of the associated alga. The lichen, according to this view, is to be regarded as a fungus that has been attacked by a chronic disease which has become generalised and necessary for the subsistence of the host-fungus. F. Moreau (1922) sums up this view as follows: 'The lichen-fungus appears as an organism characterised in its morphology by deformity due to an infective agent, an alga. The history of the association existing in lichens may be described as that of a contagious malady marked by the invasion, development, inhibition, and death of the infective agent on the one hand, and on the other hand by the morphological reactions and defensive processes of the attacked organism. In conformity with the virulence and relative immunity of the two opponents, the struggle may be short, the association transitory, the conflict may last indefinitely, and the association, rendered lasting, presents the appearance of a harmonious symbiosis.'

(2) *The Root-nodules of Leguminous and other Plants.*

A well-known example of symbiosis is afforded by the presence of the bacteroids in the nodules of leguminosæ, the micro-organisms being capable of fixing atmospheric nitrogen and thereby rendering nitrogen available for assimilation by the plant. This was demonstrated by Hellriegel and Willfahrt (1888), Schloesing and Laurent, whilst Beijerinck cultivated *Bacterium radicicola* from the nodules and produced nodules synthetically by bringing the plant and bacterium together on previously sterilised soil. According to Pinoy (1913), the bacteroids are myxobacteria, and, in the case of one species which he has specially studied (*Chondromyces crocatus*), it was found essential for the successful cultivation of the micro-organism, apart from its host-plant and in vitro, that it should be grown in association with a species of

micrococcus; similar observations have been made on other micro-organisms by bacteriologists, and some refer to the condition as one of symbiosis. Bacteriologists, I would note, are continuously misapplying the term symbiosis in referring to bacteria grown in mixed cultures, when there is no evidence whatever that the micro-organisms are mutually interdependent for their growth. In passing, it may be mentioned that nodules on the roots of the alder are attributed to the presence therein of *Streptothrices*, and that comparable nodules occur in Eleagnaceæ. The nodules on the leaves of Rubiaceæ and tropical Myrsinaceæ are also regarded as due to bacterial symbionts.

(3) *The significance of Mycorhiza in relation to various Plants.*

It has long been known that the roots of *most perennial and arborescent plants* are invaded by the mycelium of fungi known as *Mycorhiza*, and it is to Kamiensky (1881), and especially Frank (1885), to whom we owe the hypothesis that we are here dealing with symbiotic life. Frank distinguishes two forms of *Mycorhiza*: (1) the *ectotrophic*, which surround the root externally like a sleeve and are found especially about the roots of forest trees (Conifers), and (2) *endotrophic*, which penetrate deeply into the root tissue and even into the cells of the root. The *endotrophic Mycorhiza* are derived from the outside; their mycelium enters the root by penetrating the epidermal cells at the base of the root hairs, passes between the cells and into them where the mycelium branches dichotomously, and forms ultimately a much-branched intracellular growth. By this time the fungus is no longer in communication with the exterior of the root, and it nourishes itself within the host cell, only, however, by utilising the reserve substances stored there whilst avoiding the cell protoplasm or other living host elements. The host cell, after a period of inertia, exhibits a distinct reaction to the presence of the fungus, in that its nucleus becomes hypertrophied, divides repeatedly and becomes amœbiform in contour. The contained mycelial mass undergoes degeneration, is digested by the host, and the host-cell resumes its normal life. These root-*Mycorhiza* have not as yet been cultivated,[1] as have others to which reference will presently be made, and it is as yet impossible to assign them a place among known species of fungi. Further details regarding these forms will be found in the publication of Gallaud (1904).

Mycorhiza in Orchids.

The first to note the presence and to attempt to cultivate the fungus mycelium in the roots of orchids was Reisseck (1846), and in 1881 Kamienski advanced the hypothesis that the association was one of symbiosis. Wahrlich (1889) subsequently found symbionts in all species of orchids he examined, about 500 in number, thereby showing that their distribution is generalised.

It is to the researches of Noël Bernard (1902 onward), however, that we are actually indebted for the complete demonstration of the true

[1] Magrou (1921) reports that he isolated *Mucor solanum* n. sp. from *Solanum dulca-mara*, and he seems to have infected the potato plant with the fungus.

relation existing between orchids and *Mycorhiza*, based as it is upon physiological studies. All who had to do with orchids in the last century found the greatest difficulty in raising these plants from their seed; a successful result appeared to depend largely on chance. Cultivators of orchids found that success was obtained more frequently by placing seed in soil upon which orchids had previously lived, and much secrecy was observed as to the methods employed by the more successful cultivators.

The seeds of orchids are exceedingly small—a million may be found in a single capsule of an exotic species; they possess no albumen and contain an embryo consisting merely of a mass of undifferentiated cells provided with a suspensor. The essential discovery of Bernard was that orchid seeds do not germinate in the absence of fungi belonging to the genus *Rhizoctonia*. The fungus enters the seed through its least resistant and highly permeable cells, which apparently emit a secretion that attracts the fungus. Each species of orchid, according to the subsequent researches of Burgeff (1909), possesses a special species, variety, or race of fungus that is particularly adapted to it—he distinguishes fifteen species of fungus. When mutually adapted orchid seed and fungus are brought together, the mycelium of the latter penetrates the suspensor cells by digesting their cellulose wall. The mycelium traverses the epidermal cells of the seed without undergoing development within them. As soon as the primary infestation has occurred, even where the mycelium has penetrated but slightly, the cells of the seed, situated at the posterior pole of each embryo, cease to be vulnerable. In other words, a local immunity appears to be established, this immunity lasting at any rate until new regions are attacked by the fungus. This, in Bernard's experience, is the general rule. The mycelium, having attained the parenchyma cells, develops into characteristic filamentous masses recalling the appearance seen in bacterial agglutination. Nevertheless, there comes a time, this varying according to the associated species involved, when the development of the fungus is arrested by the deeper parenchyma cells of the seeds. These cells are altered before they are penetrated by the fungus; they become hypertrophied and acquire large lobose nuclei. They digest the mycelium which enters their protoplasm, but the cell continues to harbour remains of the fungus ('corps de dégénérescence') which occur abundantly in the tissues of orchids. The seed now proceeds to sprout, giving rise to a small tubercle ('protocorm'), which only at a later period produces leaves and roots.

The cultivation of Rhizoctonia of various species was carried out successfully by Bernard, the cultures being used to reproduce germination in orchids. Orchid seeds alone remained unchanged for months in cultures on agar with salop-decoction added, but when pure cultures of *Rhizoctonia* mycelium were added to such orchid seeds, the latter were invaded by the fungus, germinated, and gave rise to a 'protocorm.' Bernard gives excellent figures illustrative of the development described.

The relation between the fungi and orchids varies in different groups and plants. In primitive forms like *Bletilla* germination occurs in the absence of the fungus, but the 'protocorm' does not develop; the

rhizome, to which the plant is periodically reduced, is only periodically attacked when fresh roots are formed. *Bletilla*, however, behaves in an exceptional manner. In other orchids (*Ophrydeæ*, *Cattleyeæ*, *Cypripedeæ*, &c.) the fungus is needed for germination, and the adult plant is fungus-free except when the orchid produces fresh roots. Therefore, in such cases symbiosis is intermittent. In higher orchids like the epiphytic *Sarcanthineæ* the fungus is needed for germination, and, the roots being persistent, symbiosis is maintained continuously. Finally, in *Neottia nidus-avis* the symbiotic condition is maintained throughout the life-cycle of the orchid, the fungus being found in the roots, rhizome, and even in the flowers and seeds, and it is transmitted hereditarily.

The activity or 'virulence' of Rhizoctonia, according to Bernard, diminishes when the fungus is kept apart from the orchid, being practically lost after two or three years. An attenuated fungus regains its activity in a measure after a sojourn of some weeks in a young orchid plant; a full degree of activity under symbiotic conditions is, however, only regained slowly.

The germination of orchids in the absence of fungi was successfully induced by Bernard through cultivating them in concentrated nutrient solutions of a kind that does not occur in nature; such solutions, moreover, except under carefully carried out experimental conditions, would be rapidly vitiated through serving as a medium for the multiplication of different micro-organisms. The effect of increasing the concentration of the solution, offered to plants reared without fungi, corresponds to that obtained by raising plants with fungi of increasing activity or 'virulence.' It may be added here that when *Rhizoctonia* are cultivated on a medium containing saccharose and the substance of orchid tubers—namely, salop—they cause an increase in the molecular concentration of the medium. It is possible that the fungi, when associated with the orchids, bring about a similar increase in the molecular concentration of the sap of the invaded plant.

The Origin of Tubers in Various Plants.

The occurrence of endotrophic *Mycorhiza* in the roots of species of *Solanum* has been recorded by Janse (1897) for *S. verbascifolium* in Java, by Bernard (1909-11) for *S. dulca-mara*, by Mme. Bernard and Magrou (1911) for *S. maglia* collected in Chili, the last-named species having been regarded by Darwin as the wild type of *S. tuberosum*, our edible potato.

Experimenting with the potato, Molliard (1907, 1920) found that tubers were not formed in aseptic cultures in a poor nutrient medium, and that raising the concentration of the sugar in the sap artificially (as with the radish) led to tuberisation; concentrating the culture-medium did not induce tubers. Magrou (1921) placed potato seeds in a poor soil and close to *S. dulca-mara*, which always contains fungi, and found that only when the fungus invaded the potato plant were tubers formed.

Magrou also investigated tuberisation in *Orobus tuberosus* (Leguminosæ) and in *Mercurialis perennis* (Euphorbiaceæ), and from his collective studies the following conclusions may be drawn:—

(1) When the potato plant and *Orobus* are raised from seed, the establishment of symbiosis leads to tuberisation of the sprouts at the base of the stem; tubers are not formed in the absence of symbionts. (2) Owing to developmental differences between the two plants, symbiosis in the potato plant is intermittent, whilst in *Orobus* it is continuous. (3) It follows that these plants may develop in two ways: (*a*) when they harbour symbionts they produce perennial organs; (*b*) without symbionts they are devoid of perennial organs. (4) It is the rule for wild perennials to harbour symbionts, as Bernard has stated, whilst annuals are devoid of symbionts; three species of annuals (*Solanum nigrum*, *Orobus cœcineus*, and *Mercurialis annua*) may be penetrated by endophytes, but they quickly digest the intruders. (5) These observations confirm and supplement the view held by Bernard that tuberisation is due to the association of fungi with plants.

Mycorhiza in Ericaceæ.

Rayner (1915-16) finds that *Mycorhiza* are constantly present in heathers. He isolated *Mycorhiza* (of the genus *Phoma*) from *Calluna vulgaris*, in which the fungus is widely distributed, being found in the roots, branches, and even in the carpels, so that it occurs within the ripe fruit and seed tegument. *Calluna* seeds, when grown aseptically, give rise to poor little plants devoid of roots, but, under like conditions, in contact with *Phoma* the plants develop normally and form many roots.

Mycorhiza in Club-mosses and Ferns.

In Lycopodiaceæ (Club-mosses) and Ophioglossaceæ (Ferns), according to Bernard, the perennial prothallus is infested, and the spores whence the plants emanate will not germinate except (as with orchid seeds) with the help of fungi.

In concluding this part of my subject, dealing with symbionts of plants, I need scarcely emphasise the significance of symbiosis in the vegetable kingdom. I will close by mentioning the theoretical deduction of Bernard that vascular plants owe their origin in the past to the adaptation of certain mosses to symbiotic life with fungi.

II. Symbiosis in Animals.

(1) *Algæ as Symbionts.*

Animals of widely separated groups characterised by their green colour have long been known. Already in 1849, von Siebold attributed the colour of *Hydra viridis* to chlorophyll which, for a period, was regarded as an animal product. In 1876, Gésa Entz concluded that the chlorophyll is contained in vegetable cells living as parasites or commensals within the animals; these cells were aptly named *zoochlorella* by Brandt (1881), whilst cells distinguished by their yellow colour were subsequently called *zooxanthella*, the latter having been first described by Cienkovsky (1871) as present in Radiolaria. In the

latter case the symbionts were found capable of surviving their host, of multiplying, and of assuming a flagellate stage.

Zoochlorella occur mainly in fresh-water animals, zooxanthella mainly in marine animals, the symbionts, measuring 3-10 microns in size, being found in many Protozoa, Sponges, Cœlenterates, Ctenophores, Turbellaria, Rotifers, Bryozoa, Annelids and Molluscs.

Physiological relations between Animals and Symbiotic Algæ.—In 1879, Geddes showed that green animals give off oxygen, *Convoluta roscoffensis* (Turbellaria), when well illuminated, liberating gas containing 45-55 per cent. of oxygen. Engelmann (1881), by means of his bacteria-method, showed that *Hydra viridis* (Cœlenterata) and *Paramæcium bursaria* (Protozoa) give off oxygen when exposed to light. Geddes (1882), working with a series of marine animals, found *Velella* gave off 21-24 per cent. of oxygen, and an Actinia (*Anthoa cereus*) gave off 32-38 per cent. of oxygen. Whereas animals harbouring green algæ as symbionts always liberated oxygen, the colourless varieties of these animals never did so. Geddes regarded the association of animal and alga as being mutually helpful, the oxygen supplied by the alga to the animal and the carbon dioxide and nitrogen supplied by the animal to the alga being useful to the partners. He speaks of 'animal lichens' and 'Agricultural Radiolarians and Cœlenterates.' He found, moreover, that animals harbouring symbionts are much more resistant than those without symbionts: Medusæ (*Velella*) survived 14 days in small beakers with symbionts, only 1-2 days without them. Protozoologists have, moreover, found that Protists harbouring symbionts are easier to rear in vessels than are those without symbionts. Brandt (1883) believes that the symbionts and host aid each other in nutrition. Green *Spongilla* (fresh-water sponges) and *Hydra viridis* may live a long time in filtered water. He found that when starved green Actinia were (*a*) placed in the dark, they expelled their algæ and died rapidly, being probably poisoned by the dead algæ, but that when they were (*b*) placed in diffuse light they lived on. Actinia deprived of symbionts may become habituated in culture to live without them. Opinions (*vide* Buchner, 1921) are in conflict as to the exact relationship between the partners; in some cases (*Peneroplis* and *Trichosphærium*) the symbionts never appear to be injured, in *Amœba viridis*, &c., a limited number of symbionts are digested at all times, whereas in some Radiolarians, &c., digestion only takes place at certain stages of their development. Nutritive substances pass from the algæ into the host's cells; thus starch granules, found alongside the algæ, may be taken up by the animal cells.

Using modern methods of gas analysis, Trendelenburg (1909) concludes that green Actinias (*Anemonia sulcata*) live in true symbiosis with algæ, the algæ supply oxygen to the animal by day and at night utilise the surplus oxygen evolved, whilst carbon dioxide is furnished to the alga partly by the animal and partly by the water in which they are bathed. Rütter (1911) studied the nitrogen metabolism and concludes (*a*) that the Actinia yields to the algæ nitrogen in the form of ammonia for protein synthesis, and in darkness it adds carbon containing substances (nitrogen-free), whilst (*b*) the algæ yield to the Actinia

nitrogenous substances in the dark and by light carbon-containing substances. Organisms harbouring algæ exhibit naturally a positive heliotropism.

Symbiotic algæ are not usually transmitted hereditarily, each host-generation being usually infected afresh by algæ, encountered about the host, which may be either free-living or possess a free-living stage in their development. Exceptions occur, however, where Protozoa multiply by division and the algæ pass directly (as it were hereditarily) to succeeding generations. There are also cases of hereditary transmission in hosts that undergo sexual multiplication (as in *Hydra viridis*), the zoochlorella penetrating the egg on escaping from the host's endodermal cells after the manner of starch granules or other food reserve substances (v. *supra*). From the circumstance that in most cases symbiotic algæ are not transmitted hereditarily, we may explain the occasional occurrence of alga-free individuals in a species usually harbouring the symbionts.

Studies conducted on TURBELLARIA are of special interest: These animals may contain either green or yellow symbionts, and, as in Protozoa, some allied species harbour the symbionts and others do not. The eggs of Turbellaria are symbiont-free, each generation becoming infected afresh, the symbiont either entering the host's mouth and remaining there, traversing the intestinal wall, or entering by the genital pore, according to the particular host-species it affects.

The best-known example of symbiosis in Turbellaria is found in *Convoluta roscoffensis*, a species that has been well studied by Keeble and Gamble (1903-7). Its larvæ are colourless and infection occurs after hatching. Colourless larvæ are obtainable by transferring freshly hatched examples immediately to filtered sea-water. The cocoon, on the day following its deposition, is already invaded by many algæ having a very different structure from those found in *Convoluta*; they possess four flagella and have been referred by Keeble and Gamble to the genus *Carteria* (allied to *Chlamydomonas*). The algæ within the host possess a special structure, their contour is very irregular, they have no cellulose wall, the green colouring matter is unevenly distributed, being confined to chromatophore bodies surrounding the pyrenoid body, the nucleus is eccentric, and a number of examples are found with degenerating nuclei. Naturally all attempts to cultivate these obviously degenerating algæ have failed.

The physiological relations existing between Turbellaria and algæ differ according to the species. Thus in *Vortex viridis* symbiosis is not necessary, in *Convoluta* it is necessary for both partners. Mature *Convoluta* are never found devoid of algæ in nature. The young larva can only feed itself for a week; as it grows older it becomes infected progressively with algæ and ceases to nourish itself otherwise than upon the products of its contained symbionts. Finally, having reached an advanced age, the animal becomes capable of digesting the algæ, as does *Convoluta paradoxa* under unfavourable conditions of life. Keeble and Gamble define four periods in the life of *Convoluta*, which they term respectively hetero-, mixo-, holo-, and auto-trophic, wherein the animal

lives at the expense (1) of formed substances, (2) of these and alga-products, (3) of alga-products only, and finally (4) of the algæ themselves. This constitutes a true evolution in a species from a free existence, depending only on outside sources of food supply, to a symbiontic mode of life, and lastly one merging into parasitism.

(2) *Symbiosis in Insects.*

Among insects we find a whole series of progressive adaptations toward an association with micro-organisms of different categories:—

Group I.—*The utilisation by insects of micro-organisms cultivated by them outside their bodies.* To quote three examples: (1) The larvæ of the beetle *Xyloteres lineatus* (Bostrichidæ) form galleries in the wood of Pines. The galleries have a characteristic blue colour, produced by the growth of the fungus *Ambrosia* upon their walls, the fungus being cultivated by the larva for food. The beetle is incapable of digesting cellulose. Analogous cases occur among Ants and Termites thus: (2) *Termes perrieri* of Madagascar, studied by Jumelle and Perrier de la Bâthie (cited by Portier, 1918), builds numerous chambers and galleries. The termites collect dead wood, chew it up finely, swallow it, the wood passing unaffected through their intestine and out in the form of small spherical masses (0.5 mm.) which are cemented together as porous cakes that are impregnated with digestive secretions. Fungi which develop upon the cakes serve as food for the termites, and in well-cared-for nests the growth is harvested by the workers who triturate the mycelium and spores and feed the young larvæ therewith, whilst older larvæ receive spores, and large larvæ receive mycelium and the triturated wood contained in the cakes. (3) A third example is that of ants belonging to the genus *Atta* which cultivate fungi over areas of 5 to 6 square metres; here the queen, when about to found a new colony, carries away a small ball of fungus in a corner of her mouth wherewith to start a fresh culture in the new habitat.

Group II.—*Symbiotic organisms developing in the lumen of the intestine and its adnexa.* As examples may be cited the bacteria occurring in the intestines of fly larvæ (*Musca, Calliphora,* &c.), which aid the larva to digest meat; the bacteria associated with the olive-fly (*Dacus olea*); the Trychonymphids of xylophagous Termites (*Leucotermes lucifugus*).

Group III.—*Intestinal symbionts situated in the epithelial cells of the digestive apparatus.* The most striking instance is found in *Anobium paniceum*, a small beetle commonly occurring in flour, biscuits, dried vegetables, &c. In a part of its mid-gut are found cells filled with symbiotic yeasts undergoing multiplication (Escherich, 1900). The symbionts are not transmitted hereditarily but are acquired by the larva on hatching, being eliminated by the female beetle.

In this connection may be mentioned with reserve the observation of Portier (1918) upon xylophagous Lepidoptera (*Cossus, Nonagria, Sesia,* &c.) which, according to that author, possess intestinal fungi (*Isaria*) that multiply in the gut and form spores that penetrate the intestinal epithelium and attain the perivisceral cavity, fat-body, and

muscles of the insect. As Caullery points out, however, the supposed spores closely resemble Microsporidia, and Portier's interpretation may be erroneous. In this category also belong the symbionts described as occurring in *Glossina* by Roubaud (1919) and before him by Stuhlmann, these being found in certain hypertrophied cells of the intestinal epithelium. When liberated into the gut lumen, the symbionts are stated to multiply by budding after the manner of yeasts. Roubaud regards the yeasts as fungi, allied to the *Cicadomyces* of Šulç, and finds that they are transmitted hereditarily from the adult to the egg, larva and pupa.

Group IV.—*Intracellular symbionts of deep tissues.* This group of symbionts is most frequently found in insects, but their nature was not disclosed until recent years. Already, in 1858, Huxley described an organ which is constantly present close to the ovary in *Aphis.* Balbiani (1866) named it the pseudovitellus, or green body, and Metchnikoff (1866), who followed its development, named it 'secondary vitellus.' The function and structure of this organ were studied by subsequent authors without being understood until, in 1910, there appeared two important papers by Pierantoni (February 6), and Šulç (February 11), who demonstrated their symbiotic character, recognising the intracellular inclusions as yeasts whose evolution they completely followed. Their results have been confirmed by various authors, especially by Buchner, who in a remarkable series of papers describes a number of associations existing between insects and micro-organisms and reaches important generalisations as to their significance. It is from a collective work on the subject by Buchner (1921) that most of our information regarding this class of symbionts is taken.

Among the symbionts of deep tissues in insects are found a whole series of specialisations among the host-elements harbouring the symbionts. The least specialised instance is represented by Lecaniinæ where the yeasts are distributed throughout the body (perivisceral fluid, cells of fat-body); the fat-body cells may be regarded here as facultative Mycetocytes. In cases like *Orthezia*, symbiotic bacteria occur in certain fat cells which still contain fat droplets; this condition is also found in certain Cicadas, the yeasts being contained in fat cells which continue to accumulate fat, glycogen and urates. Finally cases occur as in Blattids where symbiotic bacteria are found in special cells greatly resembling fat cells but already forming well differentiated Mycetocytes. This class is well represented in and about the digestive tract of Pediculidæ (*Hæmatopinus*) and certain ants (*Camponotus*). Still more advanced in specialisation are those cases in which the symbiont-containing cells (*Mycetocytes*) agglomerate to form true organs termed *Mycetomas*, organs that are surrounded by flattened epithelial cells, the component mycetocytes containing either yeasts or bacteria as symbionts; such cases are found in Aphids, Chermids and Aleurodids. Mycetomas may occur singly or in numbers according to the nature of the host; the epithelial covering of the organ varies in its cell structure and pigmentation and the organ may be plentifully supplied with tracheæ whose finest branches penetrate into the interior of the mycetocytes. The relations between the mycetocytes or mycetomas and the other

organs of the host vary greatly; in some cases they occur especially in the fatty tissue, in others near the gonads, in others, as in Pediculidæ around or upon the intestine. In *Pediculus* and *Phthirus*, parasitic on man, the mycetoma is disc-shaped and lies centrally as a distinct milk-white structure upon and indenting the mid-gut. Transition forms between isolated mycetocytes and differentiated mycetomas are found in various insects.

The mode of transmission of intracellular symbionts of insects from generation to generation may take place in different ways as defined by Buchner (1921, somewhat modified):

I. The larva of each generation infects itself through the mouth (Anobiidæ).

II. Infection takes place hereditarily through the egg:

1. By symbionts set free in the blood, or which leave mycetocytes or mycetomas and attain the egg as follows:—
    - (*a*) by general infection of follicles and invasion of the egg, and finally establishing themselves at the posterior pole of the egg (Ants);
    - (*b*) by penetrating special parts of the follicles, producing for a period bacterial vegetation upon the whole egg and finally concentrating at the egg's two poles (Blattidæ);
    - (*c*) by entering the egg via its nutritive cells
        - (*a*) only some isolated fungi entering (Lecaniinæ);
        - (*b*) a number of bacteria enter in the form of a gelatinous mass (Coccinæ);
    - (*d*) by entering the posterior pole of the egg:
        - (*a*) as isolated fungi
            - (*a*) which penetrate one after another (Aphids);
            - (*b*) which accumulate in follicles and enter in a mass consisting of
                - (*a*) one kind of symbiont (Icerya);
                - (*b*) two kinds of symbiont (Cicada, Aphrophora);
                - (*c*) three kinds of symbiont (Aphalara ?);
        - (*b*) as bacteria united in several gelatinous masses (Orthezia).

2. By whole mycetomas entering at posterior pole of egg (Aleurodes).

3. By isolated symbionts leaving special mycetomas situated at juncture of follicular tubes (Pediculidæ).

III. Embryonal infection as in parthenogenetic Aphids.

It is difficult to understand the mechanism whereby the symbionts penetrate the egg in the insect's body; in any case the complicated procedure must depend upon a mutual and parallel adaptation of the insects and micro-organisms concerned.

During embryonal development the topographical distribution of the mycetocytes varies from one group of insects to another. In *Cam-*

ponotus they occur dorsally upon the mid-gut; in Blattidæ the bacteria are at first localised in the intestinal lumen, passing thence through the intestinal epithelium and entering the fat-cells. In Hemiptera and Pediculidæ the symbionts form a mass at the posterior pole of the germinal layer, and during version or unrolling of the embryo they penetrate in the ventral region of the abdomen.

As already indicated, the symbionts may be Yeasts, Saccharomycetes, Bacteria, or even Nitrobacteria. Their entrance into the cells and their presence therein even in large numbers does not in many cases prevent multiplication of the invaded cells or affect their mitosis; in other cases mitosis is more or less affected; it may become multipolar and lead to synsytium formation; and finally, cases may occur in which mitosis ceases and the symbiont-bearing cells divide amitotically.

We know little regarding the part played by symbionts in insects; our information relates almost exclusively to their morphology, mode of multiplication, and entry into the host during its development. There are no indications that the symbionts are injurious or pathogenic. It is evident, however, that they find in certain insects favourable conditions for growth, multiplication, and transmission from host to host. In these cases, therefore, we are dealing with a constant very harmonious association which excludes even a suspicion of there being any conflict between the associated organisms. We may well ask ourselves what are the reciprocal advantages of this association, but this is a question that it is impossible to answer in view of our ignorance of physiological and biochemical processes in insects.

Various hypotheses have been advanced to explain the possible function of the symbionts. Symbiotic yeasts may decompose urates (Šulç); they may produce an enzyme that aids in digestion of sugars, as in Aphids (Pierantoni); they may aid in digestion of cellulose in xylophagous insects which alone cannot render cellulose assimilable (Portier); the Nitrobacteria found in various Hemiptera may fix free nitrogen which is conveyed to them through the host's tracheæ, and thus supply the host with nitrogenous substances, thereby meeting a deficiency in its food supply.

Phytophagous Hemiptera nourish themselves chiefly upon leaf-sap without utilising the protoplasm of the plant-cells they penetrate with their sucking mouthparts. The imbibed sap is rich in mineral substances, carbohydrates and glycosides only. In these insects Peklo finds two different symbionts, Saccharomycetes and coccoid organisms, whilst Pierantoni attributes to symbionts the pigment production in *Coccus cacti*.

(3) *Micro-organisms in Relation to Luminescence in Animals.*

A fairly large number of organisms are known which have the faculty of emitting light. They are found among Bacteria, Fungi, Protozoa, Cœlenterates, Echinoderms, Worms, Molluscs, Crustacea, Insecta, Tunicata, and Fish. As a rule luminescence in animals depends upon the action of luciferase on luciferin, but recently a number of cases have become known wherein light production has been traced to micro-organisms, and it is with these cases that we shall deal.

Luminescent pathogenic bacteria may invade the host, as described by Giard and Billet (1889-90), for the small marine amphipod, *Talitrus*, of which rare light-emitting examples may be found in nature. The affected crustacean dies in about six days. The pathogenic bacterium does not luminesce in cultures, but does so when inoculated into *Talitrus*.

Luminescent symbiotic bacteria present in various light-emitting animals are, however, of direct interest to us, since their presence has been determined in luminescent organs of certain insects, cephalopods, tunicates, and fishes:

INSECTS: Pierantoni (1914) investigated the luminous organs of glow-worms (*Lampyrus*), and found them to consist of parenchyma cells crowded with minute bodies having bacteria-like staining reactions, these bodies being also present in the beetle's egg, which is luminous. He cultivated two species of micro-organisms from the organs, but does not distinctly establish their causal relationship.

CEPHALOPODS: We owe to Pierantoni (1917-20) and Buchner the discovery that luminescence in certain Cephalopods is due to light-producing bacterial symbionts living in special organs of the host. These organs may be simple or otherwise. In *Loligo* the luminous organs, hitherto known as 'accessory nidamentary glands,' represent the simpler type of organ, this consisting merely of a collection of epithelial tubes surrounded by connective tissue. In cuttle-fish (*Sepiola* and *Rondeletia*) the organs are more complicated, the glands being backed by a reflector, and provided outwardly with a lens serving for the projection of the light rays generated by the symbionts within the tubes. The symbionts are transmitted hereditarily when the Cephalopods lay their eggs. The symbionts of *Loligo* and *Sepiola* have been cultivated by Pierantoni and Zirpolo (1917-20); they inhabit the gland-tubes of the luminescent organs in large numbers, and produce light continuously, as do other luminescent bacteria in cultures.

TUNICATA: The Pyrosomidæ, all of which emit light and form tubular colonies, have long attracted the attention of biologists. Each individual in the colony possesses two fairly large luminescent organs, whose structure was studied by Panceri (1871-77), Kovalevsky (1875), and especially Julin (1909-12), who observed in the cells of the luminous organ riband-like structures appearing knotted here and there. Julin regarded the structures as mitochondria or chromidia, and it was left to Buchner (1914) to explain their true nature; they are symbiotic fungi, and are transmitted hereditarily. Buchner gives a detailed study of the symbiont and a review of the physiology of luminescence and of Pyrosomes which is well worth consulting by those interested in such problems.

FISH: Of great interest are the researches of Harvey (1922) upon light prduction by two species of fish (*Photoblepharon* and *Anomalops*) which occur in the sea about the Banda Islands, Moluccas. Their life-history is unknown. They measure up to about 11cm. in length. The author writes: 'In both fishes the luminous organ is a compact mass of white to cream-coloured tissue, flattened oval in shape, lying in a

depression just under the eye and in front of the gills. The organ looks as if made for experimentation, as it is attached only at the dorso-anterior end, and can be cut out with the greatest ease, giving a piece of practically pure luminous tissue. The back of the organ is covered with a layer of black pigment, which serves to keep the light from shining into the tissues of the fish. In both fishes there is a mechanism for obscuring the light, but, curiously enough, the mechanism developed is totally different in the two species, notwithstanding the fact that in structure the organ is identical in the two, and in every detail except proportion the fishes are very similar. In *Anomalops* the organ is hinged at the antero-dorsal edge, and can be turned downward until the light surface comes in contact with a fold of black pigmented tissue, forming a sort of pocket. The light is thus cut off. In *Photoblepharon* a fold of black tissue has been developed on the ventral edge of the organ socket, which can be drawn up over the light surface like an eyelid, thus extinguishing the light.' The histological structure of this organ was worked out by Steche (1909). The organ is continuously luminous day and night, and independent of stimulation. According to Steche, *Anomalops* constantly turns the light on and off (10'' light, 5'' dark), the fish using it, he supposes, as a searchlight to attract and mislead its prey. The natives use the amputated organ as a bait in night fishing; it maintains its luminosity for about eight hours. The organ is described by Steche as composed of a great number of sets of parallel gland tubes (acinose), separated by connective tissue, and extending across the organ from the back pigmented surface to the front transparent surface, each set arranged in a ring about a vessel which provides them with blood and oxygen. Near the surface a number of these tubes unite into a common reservoir. opening outward through a minute pore which admits sea-water. A number of pores dot the surface of the organ. The luminous material fills the lumen of the tubes; it is extracellular but intraglandular, and is never voided from the gland. Harvey states that the luminous material filling the tubes consists of an emulsion containing many granules and rods; the latter move about with a corkscrew-like motion, and are undoubtedly bacteria. The luminosity of the organ is due to these symbiotic bacteria. An emulsion containing the symbionts behaves exactly like an emulsion of luminous bacteria in being sensitive to lack oxygen, desiccation, bacteriolytic agents, potassium cyanide, &c. The continuity of the light, independently of stimulation, is characteristic of luminous bacteria and fungi alone among organisms; this, and the circumstance that luciferin and luciferase could not be demonstrated, all go to confirm the correctness of Harvey's conclusions regarding the cause of luminosity in these fish, notwithstanding that he has failed hitherto to cultivate the bacteria found in the luminous organs.

In concluding this section dealing with light production by animals it may be repeated that we have to distinguish between (*a*) luminescence due to symbiotic organisms, such luminescence being continuous in the presence of oxygen as in cultures of luminous bacteria (of which some thirty species are known), and (*b*) that due to animal cell-products known as luciferin and luciferase which are secreted and expelled

at intervals, in response to a stimulus, from two kinds of gland cells, the secretions, when mixed, producing light.

Portier's Hypothesis.

The numerous cases in which symbiosis occurs in nature have naturally led some biologists to ask if symbiosis is not a phenomenon of general significance, and perhaps essential, in living organisms. In this connection reference must be made to the hypothesis advanced by Portier (1918), because it formulates extreme views. Starting from his studies of symbionts of leaf-mining caterpillars (*Nepticula*) and wood-devouring insect larvæ (*Cossus*, *Sesia*, &c.), he sought to verify the work of Galippe (1891-1918) on micro-organisms occurring in vertebrate tissues. Using methods he supposed to be adequate, Portier claimed that he could isolate various micro-organisms from vertebrate tissues. On faulty premises he built up an hypothesis that may be likened to a house of cards. He divides living organisms into two groups, *autotrophic* (bacteria only) and *heterotrophic* (all plants and animals), according as they are provided or not with symbionts. Whereas some symbionts are cultivatable, others have become so domesticated in respect to their hosts that they cannot be separated from them. The essential function of symbionts is to elaborate reserve substances so that they become assimilable to the host cell. The mitochondria that are present in all plant and animal cells, though not cultivatable, are, according to Portier, nothing but symbionts, the importance of their function having recently been revealed by Guillermond, Dubreuil, and others.[2] They are derived from food, and, if absent therefrom, illness supervenes, as shown by the bad effects of sterilised food, decorticated rice, &c., causing deficiency diseases attributed to lack of vitamines, which, according to Portier, are nothing but symbionts. Where, as in *Aphis*, the animal feeds on plant sap that is filtered through a tube formed by the insect's saliva—in other words, the insect imbibing food devoid of symbionts—the animal is of necessity provided with its own well-developed store of them. Portier applies his hypothesis to such varied problems as fecundation, parthenogenesis, tumor-formation, variation, and origin of species, in all of which mitochondria, that is, his supposed symbionts, play a part. His views aroused great controversy in France, so much so that it was thought necessary for the Société de Biologie de Paris (see C.R. Soc. Biol. LXXXIII., 654, May 8, 1920) to have a Committee examine the evidence. The Committee, consisting on the one part of Portier and Bierry, and on the other of Martin and Marchoux (Institut Pasteur), by its report indicates the pitfalls, well known to bacteriologists, into which Portier was led, and thus disposes of the greater part of his far-reaching hypothesis. Nevertheless, like many exploded hypotheses, that of Portier has served a useful purpose through the discussion it has provoked and the interest in the subject of symbiosis which it has stimulated.

[2] Guillermond has shown that the mitochondria of the epidermal cells in *Iris* elaborates amyloplast and finally starch. Dubreuil (1913) found that mitochondria elaborate the fat in fat-cells. Other cytologists have shown that glandular secretions are similarly referable to mitochondria.

Conclusion.

The term 'symbiosis' denotes a condition of conjoint life existing between different organisms that in a varying degree are benefited by the partnership. The term 'symbiont,' strictly speaking, applies equally to the partners; it has, however, come to be used also in a restricted sense as meaning the microscopic member or members of the partnership in contradistinction to the physically larger partners which are conveniently termed the 'hosts' in conformity with parasitological usage.

The condition of life defined as symboisis may be regarded as balancing between two extremes—complete immunity and deadly infective disease. A condition of perfect symbiosis or balance is realised with comparative rarity because of the many difficulties of its establishment in organisms that are either capable of living independently or are incapable of resisting the invasion of organisms imperfectly adapted to communal life. In these respects the conclusions of Bernard and Magrou in relation to plants apply equally to animals. It is difficult to imagine that symbiosis originated otherwise than through a preliminary stage of parasitism on the part of one or other of the associated organisms, the conflict between them in the course of time ending in mutual adaptation. It is, indeed, probable that some supposed symbionts may prove to be parasites on further investigation.

In perfect symbiosis the associated organisms are completely adapted to a life in common. In parasitism the degree of adaptation varies greatly; it may approach symbiotic conditions on the one hand, or range to vanishing point on the other by leading to the death of the organism that is invaded by a highly pathogenic animal or vegetable disease agent. There is no definite boundary between symbiosis and parasitism. The factors governing immunity from symbionts or parasites are essentially the same.

No final conclusions can as yet be reached regarding the function of symbionts in many invertebrate animals owing to our ignorance of the physiological processes in the associated organisms. The investigation of these problems is one fraught with difficulties which we must hope will be surmounted.

New knowledge is continually being acquired, and a glance into new and even recent publications shows that symbionts have been repeatedly seen and interpreted as mitochondria or chromidia. Thus in *Aphis* the long-known pseudovitellus has been shown to contain symbiotic yeasts by Pierantoni and Šulç, independently and almost simultaneously (1910); Buchner (1914) has demonstrated symbiotic luminiscent fungi in the previously well-studied pyrosomes, besides identifying (1921) as bacterial symbionts the mitochondria found by Strindberg (1913) in his work on the embryology of ants. The increasing number of infective diseases of animals and plants, moreover, which have been traced, especially of recent years, to apparently ultramicroscopic organisms cannot but suggest that there may exist ultramicroscopic symbionts.

From the foregoing summary of what is known to-day of symbiosis

we see that it is by no means so rare a phenomenon as was formerly supposed. Symbiosis occurs frequently among animals and plants, the symbionts (Algæ, Fungi, Bacteria) becoming in some cases permanent intracellular inhabitants of their hosts, and at times being transmitted from host to host hereditarily. Among parasites, non-pathogenic and pathogenic, we know of cases wherein hereditary transmission occurs from host to host.

It is evident that we are on the threshold of further discoveries, and that a wide field of fruitful research is open to those who enter upon it. In closing, it seems but fitting to express the hope that British workers may take a more active part in the elucidation of the interesting biological problems that lie before us in the study of symbiosis and the allied subject of parasitism.

Acknowledgment.—I have pleasure in expressing my thanks t colleague, Mr. David Keilin, for the very valuable aid he has me in the preparation of this address.

SECTION J.—PSYCHOLOGY.

THE MENTAL DIFFERENCES BETWEEN INDIVIDUALS.

ADDRESS BY

Dr. CYRIL BURT, M.A.,

PRESIDENT OF THE SECTION.

The most remarkable advances made by psychology during recent years consist in the rapid development of what threatens to become a new and separate branch of science, the study of individual differences in mind. Down to the close of the nineteenth century psychologists were all pure psychologists. They confined themselves, with an air of chaste aloofness, to the discussion of mind in general; they wrote and experimented solely on the abstract functions of consciousness as such. The varying eccentricities of minds in the concrete, how one man's consciousness might be unlike another's, were problems beneath their interest or beyond their ken. If, in some laboratory research, different persons gave dissimilar results, either in the sharpness of their senses or the speed of their reactions, the divergencies were treated as no more than unavoidable disturbances of measurement, vexatious errors to be eliminated by the method of averaging, not facts of special value to be examined in and for themselves. For the rest, the chief method of the psychologist was still introspection; and his chief subject, himself. Accordingly, although in this way he laid the necessary foundations of a sound terminology and a safe technique, he nevertheless exposed himself to the taunts of his literary colleagues, who knew that it takes all sorts to make a world. 'Les philosophes' (laughs an early and unorthodox observer) 'sont toujours trop occupés d'eux-mêmes pour avoir le loisir de pénétrer ou de discerner les autres.'[1]

Of late, however, a body of workers has arisen who have turned their attention more especially to the peculiarities of particular minds. The variations have attracted them more than the averages; and the mental disparities between childhood and age, between race and race, between one sex and the other, and between each unique individual and the rest, have formed their chosen topic. As a result of their labours, there has grown up, step by step, a vast and miscellaneous accumulation of data which urgently demands to be sifted and systematised. The practical needs of applied psychology, in each of its fresh spheres—the psychology of war, of education, of industry, of mental disorder, deficiency and crime—all depend for their solution upon a sound doctrine of individual differences; and their study in its turn has already contributed much welcome information to the parent science. I propose, within

[1] La Bruyère, *Les Caractères* (1687).

the limits of the time allowed me, to attempt a summary of the chief problems and principles of this new branch; and, as methodically and as completely as is possible within so narrow a compass, to plot out the ground explored by recent work.

Though the scientific study of individual minds is new, the popular interest in the practical issues has a long and venerable record; the ancient title of 'psychology' is by comparison a word of yesterday.[2] Time after time in the history of knowledge, the quack who has pandered to a public want proves to have been the primitive precursor, the earliest *avant-coureur*, of what afterwards arrives as a respected and respectable science. Astrology was the forerunner of astronomy, alchemy of chemistry, and the lore of the bone-setter and the herbalist of modern surgery and medicine. In the same way the charlatan who reads your character from the lines on your hand or the bumps on your skull is carrying on an antique tradition which embodies the first attempts at a psychology of individuals. He has seen the problem; he has met the demand; and, if his wares are sham and shoddy, he has at least thrown down a challenge to the slower and more scrupulous disciple of truth.

The blunders of pseudo-science, however, are never wholly uninstructive. Those who first practised *l'art de connaître les hommes*—the physiognomists, the phrenologists, the palmists, and their successors—were all, in their crude and curious speculations, mainly guided, and mostly misled, by three fallacious assumptions. They looked for nothing but permanent and external signs; they assumed nothing but constant connections between the outward and visible symptom and the inward and invisible state of mind; and they classified both physical symptoms and mental qualities into arbitrary and discrete types. Thus, your nose was either pointed or not pointed; and your temperament was either choleric or not choleric. If your nose was sharp, then your temper must be sharp as well: for *nasus acutus irascibiles notat*. No graduations were recognised; no exceptions admitted. The correspondence was made perfect and invariable. Indeed, if the classes were not clear-cut, if the symptoms were not patent to superficial inspection, and if the connections between the two were not absolute and uniform, how could there be any inference, any prediction, any science of whatever sort? The soul, surely, must be a riddle which could never be read.

The difficulty was solved by the discovery of a new technique. And this we owe to an original English thinker of the latter half of the nineteenth century, Sir Francis Galton. To the general public, Galton is best known by his demonstration of the hereditary factors in individual genius—a doctrine that in his own person he so remarkably exemplified.

[2] I suppose the earliest written recognition of the power of judging the quality of the mind from observable characteristics is to be found in the words of Nestor to the unknown Telemachus: 'By certain signs that I discern upon thy face, illustrious youth, I recognise what man thou art. Thy countenance is proud and generous, thine eloquence great, and thy reasoning recalls to me thy father. What manner of youth could such a one as thou be, were he not the offspring of the great Ulysses?' Homer, *Odyssey*, xi., 693. Those who care to trace the historical development of individual psychology will find most of the necessary materials and references collected in Mantegazza's *Physiognomy and Expression* (1904) and Stern's *Differentielle Psychologie* (1911).

But his most fruitful contribution lay in the development of two technical methods of inquiry, the statistical method of correlation, and the experimental method of psychological tests. These in turn rest upon a fundamental assumption, which recent work has verified—*the continuity of mental variation.* Here stands the keystone of individual psychology as a science. The differences between one man and another are always (we shall find) a matter of 'more or less'—seldom, if ever, a question of presence or absence, of 'all or none.'

'Virtuous and vicious ev'ry man must be,
Few in th' extreme, but all in a degree.'[3]

There are, in fact, no such things as mental types; there are only mental tendencies. And it becomes the main task of individual psychology, first, to catalogue and classify all the tendencies to be surveyed, and then to devise a method for the quantitative assessment of each.

It follows from this initial postulate that the mind of every individual has the same underlying structure. Men's minds are like their faces. Each seems at first unique. But patient analysis shows that the real component features are in every one the same. All have two eyes, two ears, a mouth, a forehead, and a nose. But the length, the width, and the prominence of each part may differ infinitely from man to man. Our business is thus to calculate the extent to which each known potentiality has been developed or contracted, much as a surveyor marks down, at given stations upon his map, the eminences and depressions of the land.

The Psychographic Scheme.

Since the mental ground-plan is in all persons approximately the same, the same inventory of mental tendencies will serve, no matter which particular person we are about to analyse. An identical set of questions may be asked about each. An identical series of headings will recur in our reports. Were our psychological catalogue exhaustive and complete, it would, in theory, be necessary only to measure in succession each particular capacity; and so obtain a clear and quantitative specification of the idiosyncrasy of each individual.

This view is more than a mere dogmatic postulate. It is confirmed by a close comparison of the literature in different psychological fields. It will be discovered that, whatever the nature of the case to be examined—mental deficiency or supernormal talent, educational backwardness or vocational misfit, neurotic disorder or propensity to crime—practical experience has forced each examining psychologist to work out very much the same main heads of inquiry as his colleagues in other lines. Such a working schedule of mental characteristics may be termed a 'psychographic scheme.' The scheme that I shall follow here will be one which I have found reappearing as a basis for my note-taking in investigating individuals in each of the foregoing groups. In its broadest outlines every personal examination should pursue two chief directions: first, a *retrospective* inquiry into the past history of the person studied; and, secondly, what I may call a *conspective*

[3] Pope, *Essay on Man*, 231.

survey of his mental condition at the present time. Viewing his whole life's story as a growing tree with ramifying roots and boughs, we take, as it were, first, a longitudinal section, and then one or more cross-sections, of the main trunk.

I. *Case-History.*

The historical retrospect should embrace, first of all, a *personal history*, based upon reports supplied by parents, teachers, and medical attendants, and reviewing such developmental features as conditions of birth, mental and bodily growth, past physical ailments, and early mental shocks and disorders, and general moral and intellectual progress both at home and at school.

The procedure of the modern psycho-analyst consists in little more than the taking of an elaborate mental case-history by means of a special technique. The discovery of early repressions and infantile complexes often sheds a bright flood of light upon the present mental make-up of an adult person. Think, for example, what numerous characteristics may be explained when it is reported that a neurotic bachelor of thirty was the only son of his mother, and that she was a widow. Mill, who in this country was the first to raise the science of character to a level of philosophical respectability, regarded 'ethology,' as he named it, as consisting principally in the deduction of present mental features from past encircling influences.

But the inquiring psychologist must go further back still. He must pass behind birth to ancestry; and to the personal history of his subject prefix an account (where he can get it) of the *family history*. Here he obeys the lead of Galton rather than the logic of Mill; and is seeking, by a study of pedigrees, to infer the presence of hereditary factors. Of these the ultimate significance will be presently apparent.

II. *Personal Examination.*

What I have termed taking a cross-section must include an examination of the person's present condition by the two chief instruments of all scientific inquiry—namely, observation and experiment. By whichever method they are reached the facts established will be brought together synoptically under a convenient system of tabulated heads. These headings will embrace external conditions as well as internal, and physical conditions as well as mental

A. *Environment.*

The psychologist must never be content to look at nothing but the mind before him. It is his task to extend his survey to the surrounding influences that are making that mind what it is; he must ascertain the current situations and the crucial problems which that mind is called upon to meet. To study a mind without knowing its *milieu* is to study fishes without seeing water.

Accordingly, as he turns from the past to the present, the human naturalist will commence with a review of the person's present environment, of his material, physical, and moral circumstances, at home, at school, and at business. Recent research upon milder abnormal states

—particularly delinquency and neurosis—has shown very clearly that none of these is due exclusively to inborn constitution, nor yet entirely to shocks and mental traumata in the remote or immediate past; they spring largely out of contemporaneous conditions and conflicts. Even with the normal individual, simply to learn in what social class he moves, or in what city or street he lives, is to divine very plausibly the chief of his guiding habits and ideas—his code, his creed, and his customs. Strange persons are like strange words: their intentions are best guessed from their context. One incidental item of practical import rises to the surface in most of these investigations. Of all external factors home influences are paramount; moral influences are far more powerful than material, emotional than intellectual. Without a knowledge of the emotional attitudes elicited in a person by the attitude of his parents, of his various relatives, and of others in daily contact with him, his standpoint towards life can never be properly envisaged or explained.

B. *Personality.*

1.—*Physical Condition.*

Turning from the environment to the personality proper, from the setting to the gem, it is essential to glance first at physical conditions before we pass to psychological; to see what is reflected on the surface before we hold the centre to the light. That a man's body has a profound influence upon his mind has been realised in every age. But we are only just beginning to discover in definite detail how certain physical states and certain physical disorders are attended by certain psychical effects.

Once more it is in pathology—where more or less morbid conditions of body produce more or less morbid conditions of mind—that the most convincing instances are to be seen. At present, it is true, the tendency in the newer schools of psychology is to trace mental derangements, particularly in their milder forms, almost exclusively to mental origins. But those who deal daily with young children, where the causal factors can be more readily unravelled, find it impossible to overlook the co-operation of such purely physical conditions as rheumatism, chronic catarrh, nasal obstruction in numerous forms, minor lesions of the brain, or the absorption of toxines from internal foci or superficial sores.

The study of juvenile delinquency shows, in most unexpected directions, the influence of physique upon character. Anything that weakens physical health tends to weaken self-control. Anything that conduces to physical irritation tends to set up a mood of mental irritability. A holiday in the country is sometimes the best cure for crime. With the intellectually subnormal the efficacy of simple physical remedies is quite as striking as with those who are subnormal in character or temperament. The provision of spectacles, the extraction of teeth, the extirpation of tonsils and adenoid-growths, measures in themselves comparatively trifling, have often converted an alleged mental defective into a normal or nearly normal child.

Of all the physical influences studied in recent years the most striking is that of the ductless glands.[4] Every layman knows that thyroid insufficiency produces a cretinous type of mental defect, and that such defect may be cured or alleviated by the administration of glandular extracts. And just as thyroid insufficiency depresses, so thyroid excess may heighten, emotional states and reactions. Of other glands belonging to this class—the pituitary, the adrenal, and the sex glands—we know far less. But recent work upon their internal secretions has left no doubt as to their power over temperament and feelings. Shall we some day, when biochemistry is sufficiently advanced, be able to analyse the minute components of lymph and blood, and diagnose from the chemical constitution of small samples whether a man is oversexed, or easily fatigued, timorous, excitable, or blessed with high vitality?

The work upon these endocrine organs seems destined at length to provide a scientific basis for the doctrine of physical signs—the tradition so dear to the popular mind-readers of every place and time. The physical signs recommended for inspection are of two kinds: they refer either to the physique as a whole, or more specifically to the face or head.

The ductless glands are closely connected with body metabolism as a whole. We seem here to find an unexpected confirmation of the popular division of 'temperaments' or 'constitutions' into two or three chief types. The loose terms in vogue are, for the two extremes, 'nutritional' or 'vital,' and 'nervous' or 'mental'; and, for the intermediate, 'motive,' 'muscular,' or 'mixed.'[5] Three American physiologists—Bryant, Goldthwait, and Dunham—whose observations on this point are more careful than most, quaintly term the triad 'herbivorous,' 'carnivorous,' and 'omnivorous' respectively, thus claiming a somewhat speculative biological derivation for the supposed differences in digestion, metabolism, and general manner of life.[6] Three Italian physiologists, Viola, Naccarati, and De Giovanni, term the two extremes—the Hamlets and the Falstaffs of the psychological caste—microsplanchnic and macrosplanchnic respectively, or (in language less technical but more Shakespearean) little-bellied and big-bellied. By means of careful statistical correlations they have tried to prove that the ratio of height to weight, or better of limbs to trunk, may be taken as a trustworthy index of the so-called 'morphological type,' and is

[4] It is unfortunate for the general reader that the only systematic and non-technical account of the subject is the somewhat uncritical book by Dr. Berman on *The Glands Regulating Personality*, a work as full of ingenious speculations as it is devoid of documented references.

[5] This threefold division is found in most phrenological handbooks. Of these the least unscientific is Dr. Bernard Hollander's *Scientific Phrenology* (a title which is something of a *contradictio in adjectivo*); see pp. 38-48. The distinction, in its modern form, seems to have originated with Dr. Alexander Walker, a Lecturer on Anatomy at Edinburgh University, and contemporary of the English phrenologist Combe. It will be observed that the dichotomy is apparently a simplification of the fourfold classification of temperaments, originating with Galen (A.D. 130).

[6] For a convenient summary of the American literature see Lewis, 'Adolescent Physical Types,' *Ped. Sem.*, 1916, xxiii., 3.

demonstrably associated with tested mental differences.[7] Fat and lean is an antithesis as old as the legend of Jack Spratt and his wife; and modern physiology, it will be noted, agrees with the ancient rhyme in referring the difference largely to dietetic habits, and in connecting it in part with a difference in sex and the sex-glands. As to the concomitant psychical differences, fancies on this subject (if Plutarch is to be trusted) were entertained by so eminent a master of men as Julius Cæsar.[8] 'Your fat, sleek-headed men,' he is made to exclaim, 'I never reck of; they sleep o' nights. But these pale-visaged carrion, with the lean and hungry look, they think too much; such men are dangerous.' That the new observers have confirmed the old is more than I venture to assert. But at least they have applied the proper method to the problem.

Of their somewhat singular conclusions the real import lies in this: they emphasise, and justly emphasise, the supreme importance, for right psychological diagnosis, of viewing body and mind as a single unitary organism. A man is something more than a carcass loosely coupled with a ghost. Material and spiritual are reciprocally involved; and the two together are to be treated as inseparable aspects of one highly complex whole. Thus, in both physical and mental working, the restless, unreliable, 'carnivorous' type may be likened to a high-compression engine, capable of short but forcible output of energy, yet unsuited for long and steady running; the plodding, sedentary, 'herbivorous' type, to a low-compression engine, with a lower maximum efficiency, but a more continuous level of sustained activity. And in each the mental and physical symptoms are joint products of one fundamental mechanism. It will be remarked in passing that, alike in mind and body, the former—the slender 'microsplanchnic' type—is suggestive of hyperthyroidism, and of the tall, long-headed, active races; while the latter—the heavy 'macrosplanchnic' type—is similarly suggestive of hypothyroidism, and of the short, round-headed, stolid race.

Possibly the same twofold hypothesis—of racial stock and glandular influences—may be adduced to explain what little correlations the phrenologist[9] can claim between mental characteristics and the conformation of skull and face. The appearance of cranial types is certainly suggestive of what is known of racial stocks. The doctrine of stigmata of degeneration also finds a partial explanation in the double effects of disturbances in the ductless glands, impairing simultaneously the normal development of both skeleton and intelligence. Low, narrow, and bossed foreheads, broad, depressed, and upturned noses, narrow, high, and V-shaped palates, lobeless, projecting, and malformed ears, asymmetrical, misshapen, and small skulls—these

[7] See Naccarati, 'The Morphological Aspect of Intelligence,' *Arch. Psych.*, No. 45, 1921. The coefficients are low, .35 or less.

[8] The remark is freely paraphrased by Shakespeare, *Julius Cæsar*, I., ii., 191.

[9] Psychologists will be astonished to hear that in spite of all the recent work on intelligence-tests, one British Education Authority recently preferred to invite a practising phrenologist to assist in the examination of candidates for junior county scholarships. How many school medical officers still rely, in diagnosing mental deficiency, more upon stigmata than tests?

anatomical disfigurements were, until recently, the chief signs relied upon in the diagnosis of mental defect. They are best seen in the rarer clinical types of imbecility, in the mongol and the cretin, who, as already remarked, seem to suffer primarily from a deficiency of endocrine secretions.[10]

So far, it may be thought, bodily indications are of value only in cases of extreme pathological deviation—the obese, the emaciated, and the physically deformed; they are symptoms for the doctor, not signs for the plain man. Is there, then—

'. . . no art
To find the mind's construction in the face'?[11]

And, if not, why do so many men and women of the world claim to divine character at a glance, and profess, on the basis of a first impression and a short superficial inspection, to gauge intelligence and temperament, even among their normal fellow-creatures, with much the same exactitude which is conceded to the dog-fancier, the sheep-dealer, and the fellow with an eye for horseflesh in their somewhat lowlier spheres? That their intuitions (as they term them) often correlate highly with independent and trustworthy estimates has been shown statistically time after time. Upon what do they rely? Is there a sort of moral clairvoyance confined only to a gifted few? Or is the miracle of insight into another, a knack that each can achieve? In part these judges of men are aided, more than they themselves suspect, by semi-social criteria—accent, phraseology, manners, the elegance of handwriting, and the tidiness of clothes. Stevenson, you will remember, has declared that 'an undoubted power of diagnosis rests with the practised Umbrella-philosopher; for, whereas a face is given us ready-made, each umbrella is selected from a shopful as being most consonant with the purchaser's disposition.'[12] And other interviewers, besides Sherlock Holmes, draw unpalatable inferences from our taste in hats, and socks, and coloured ties. For the rest, so far as their procedure is unprejudiced by pseudo-scientific reading, it seems to depend chiefly upon inferences, conscious or unconscious, not so much from bodily structure or build, as from bodily posture and movement, particularly the finer movements of the hand, of the eye, of the lips and mouth, and of the vocal organs in speech. And the principle is sound. If you are buying anything that works you ask first to see it working, be it only for a second, and only as a sample. So with the connoisseur of human creatures, it is function rather than framework that should count. In the face, it is not the hard, immutable gristle and bone, but the soft and mobile mask of muscle that the sound

[10] Many of the so-called stigmata, however, together with the mental dulness they are supposed to signify, are largely attributable to petty ailments of early childhood—rickets, chronic respiratory catarrh, and nasal obstruction from adenoid growths.

[11] *Macbeth*, I., iv., 12.

[12] *College Papers*, iv., 'The Philosophy of Umbrellas.' As to handwriting, those who smile at the claims of the graphologist may be reminded that Binet, and many experimentalists and pathologists since, have not scorned to look for indications of character and mental derangement in the size, and shape, and steadiness of the letters we trace with the pen.

physiognomist observes. The neuro-muscular tonus—the tightness around the eyelids, the firmness of the lips—is an index of the general state of health and vitality upon which a man's intelligence and attention so much depend. The changes of look and glance afford a clue, however indirect, to the range, the liveliness, and even the character of his interests. Above all, it is to be remembered, almost every human emotion has its instinctive facial expression, to which, by a sort of primitive sympathy, we ourselves as instinctively respond. The emotions (we shall see) are the foundations of character. And the emotional mood that predominates in a given person's life tends, by the simple law of habit, to leave its natural expression stamped upon the countenance, contracting almost permanently the underlying muscles, and deepening the furrows and the finer lines upon the skin. Thus the bad-tempered bully comes to wear always a more or less angry scowl, and the anxious melancholic a worried look upon the brow.[13]

'In many's look the false heart's history
Is writ, in moods, and frowns, and wrinkles strange.'[14]

In the main, however, the gist of recent scientific work on connections between body and mind has been, from a practical though not from a theoretical standpoint, negative. Theories, such as that of Lombroso and his school—the notion of criminal, defective, neurotic, and supernormal types, each marked off from ordinary mankind by a specific combination of physical and mental traits—have been exploded by more careful statistical methods. The measurable correlations, though frequently positive, are almost always too slight to be trusted for the needs of diagnosis.[15] Thus a man's exterior is sometimes suggestive, but never conclusive. And so we reach the safe and central maxim of individual psychology of to-day: *Judge mental functions by mental symptoms, not by physical.* The worldly moralist agrees. 'Il ne faut pas juger des hommes comme d'un tableau ou d'une vache; il y a un intérieur et un cœur qu'il faut toujours approfondir.'[16]

2.—*Mental Condition.*

I proceed now to what consequently becomes the essential duty of the practical psychologist—the direct examination of the mental state.

The positive foundations for a practical psychology of individual differences have been laid in three broad generalisations, each the separate suggestion of recent experimental work. They consist in a trio of important distinctions, the distinction between intellectual and emotional characteristics, between inborn and acquired mental ten-

[13] These deductions can be verified by the method of correlation, see *Child Study*, June 1919, 'Facial Expression as an Index of Mentality'; also Langfeld, 'Judgments of Emotions from Facial Expression,' *J. Abn. Psych.*, xiii., 172, and *Psych. Rev.*, xxv., 488. The general principle underlying 'whatever truth the so-called science of physiognomy may contain' is stated, as in the text, by Darwin, *Expression of Emotions in Man and Animals*, p. 388.

[14] Shakespeare, *Sonnets*, xciii.

[15] The labours of Karl Pearson and his students, following the methods of Galton, have been invaluable in this field. Goring's study of *The English Convict* is a model for inquiries upon these and kindred problems.

[16] La Rochefoucauld, *Maximes Morales*, cccxvii.

dencies, and between special and general capacities. Upon these three distinctions the essential portion of my 'psychographic scheme' is based. The evidence for them, as yet presumptive rather than complete, I can but briefly touch upon in the appropriate place.

a.—*Intellectual Characteristics.*

With these distinctions, then, to mark our working rubrics, we begin by viewing any particular mind, that comes for valuation, as presenting two distinguishable aspects, the intellectual, on the one side, and the emotional on the other. The divorce of νοῦς from θυμὸς is as old as Pythagoras.

'Two principles in human nature reign;
Passion to urge, and reason to restrain.'

The modern antithesis is something more than a convenient revival of the traditional contrast. It has a basis in recent statistical work.[17] If a large group of individuals be ranked in order for all the psychological characteristics that can be conceived or at least conveniently estimated, and if the correlations between the several rankings, each with each, be then computed, two striking facts are instantly perceived. First, nearly all the correlations are positive; excellence in one respect tends, on the average and in the long run, to go hand in hand with excellence in every other. But, secondly, the closeness of this correspondence varies suggestively in different directions. Intellectual qualities are correlated fairly highly amongst themselves. Emotional qualities (so far as the more meagre evidence at present shows) are likewise correlated to nearly the same considerable degree. But the correlations between intellectual qualities on the one hand and emotional on the other, though still as a rule positive, are by comparison conspicuously low. We are warranted, therefore, in assuming that these two aspects are relatively independent, and in studying them separately and in succession.

i. *Inborn Abilities.*

We proceed then to estimate, in the first place, the examinee's qualities of intellect. And here our second subdivision introduces itself—the distinction between what is inborn and what is acquired. Many independent researches agree in showing that intellectual characteristics are hereditary, and that to much the same extent as physical. Even if a capacity (or, more strictly, the strength of a capacity) be not hereditary, it may still be congenitally determined. What is inherited is necessarily inborn; but what is inborn is not necessarily inherited. In the latter case, however, to separate endowment from acquirement, mental capital from mental earnings, is a more precarious task. The discrimination, wherever it is possible, is of the greatest practical moment. If a child, for example, proves to be exceedingly backward in school work it is essential to decide whether this backwardness is a legacy from backward ancestors, or merely an

[17] See amongst other studies, Webb, 'Character and Intelligence,' *Brit. J. Psych. Mon.*, I.

accidental consequence of conditions subsequent to birth. In the former case the backwardness, being inherent, is therefore incurable; in the latter, there remains at least a hope that, by amending the unfavourable circumstances, the backwardness may be partly remedied or even wholly removed.

a. General Intelligence.

We have now narrowed our scope for the moment to qualities that are intellectual and at the same time inborn; at this point we may apply our third and last distinction. Inborn intellectual abilities are divisible, first, into a single central capacity, pervading all that we say or think or do; and, secondly, into a series of specific abilities, each entering only into processes of a more or less limited kind.

For the existence of general inborn intellectual ability (known briefly as 'intelligence') the statistical evidence is now pretty decisive. Even the critics of this so-called central factor no longer deny that, at least as a matter of mathematical interpretation, the empirical data may be formulated in these terms; and that this formulation, whatever its ultimate psychological explanation, is of the greatest value in practice, and, as a working hypothesis, works very well.

If further proof were demanded, the indubitable success of intelligence-testing has supplied a widespread verification, sufficiently business-like to convince the plain man. Indeed, over the whole realm of mental science the outstanding feat of recent years has been the application and the multiplication of innumerable tests for measuring general ability. As everybody knows, during the War the intelligence of nearly two million recruits was tested by these means for the Army of the United States. And this spectacular achievement has probably bestowed on the practical applications of psychological methods a stronger impetus than any other single piece of work.

Since intelligence, as we have defined it,[18] is an inborn quantity, the amount possessed by a given individual should, in theory, remain constant through all the years of his life. It should thus be possible to predict, from quite an early age, what will be the probable intellectual level of a child when he is grown up. Within reasonable limits such forecasts can, in fact, be made. Numerous investigations have shown that what is called the 'mental ratio'—the proportion, that is, between a child's mental age and his chronological age—tends to keep tolerably uniform throughout the years of growth. Hence it is safe to prophesy that a child (for example), aged five by the calendar, with a mental age of two (and a mental ratio, therefore, of $\frac{2}{5} = 40$ per cent.), will probably attain a mental age of four at the age of ten, and a mental

[18] The reader will understand that intelligence in this sense is not to be conceived as a concrete organ, entity, or power, but a purely abstract potentiality—like electrical energy or heat as conceived by the physicist—an entirely hypothetical quantity, postulated and defined, like most other scientific concepts, for the convenience of separate measurement. It is to be distinguished from manifested intelligence (the materialisation, as it were, of that abstract potentiality) which develops during childhood and decays with loss of health or advance of age, and is measurable in terms of mental years or of some more concrete unit.

age of six at the age of fifteen. Since beyond the stage of puberty inborn intelligence does not develop to an appreciable extent (another startling paradox of psychological testing), such a person will never rise above the six-year level, and will remain mentally defective for the rest of his life.

From the numerous results obtained from the widespread employment of intelligence-scales, one fact of deep social significance emerges—the vast range of innate individual differences. A famous clause in the American Declaration of Independence proclaims that 'all men are created equal.' In the psychological sense as distinct from the political, not only are men created unequal, but the extent of the inequality surpasses anything before conjectured. In a survey carried out upon all the children in a representative London borough—a census covering more than 30,000 cases—it was found that, within the elementary schools, the mental ratios might vary from below 50 per cent. to above 150 per cent.; that is to say, the brightest child at the age of ten had the mental level of an average child of fifteen, while the dullest had the mental level of a little child of only five.[19]

Over this vast scale the distribution of intelligence is neither flat nor yet irregular; it follows a simple mathematical law. Its frequency conforms to the so-called 'normal curve,' and the abnormal and defective are found to constitute no isolated types, but to be simply the tail-end of a chance distribution. Probably all or most of our mental capacities are distributed in the same fashion. This fact, if it be a fact, greatly simplifies the problem of mental measurement. It should be a recognised maxim of procedure to measure people, not by arbitrary marks between a conventional zero and an equally conventional maximum, but by the degree of divergence above or below the average or middle line (much as we measure the depth of the ocean or the altitude of the hills from the intervening sea-level), the divergence being calculated in terms of the standard deviation. This is a technical hint of special value in estimating qualities that lend themselves to no obvious quantitative units like mental ages or additive marks.

Since variations in intelligence are so wide and so continuous, it becomes convenient to divide the entire population into about six or eight separate classes or layers. A classification of this kind, worked out empirically, for children, is already implicitly embodied in the organisation of our various schools. A second classification can be drawn up, on an analogous basis, for adults, and will be found, in the main, to reflect the amount of difficulty and responsibility entailed by their several occupations. It is interesting to find that the proportionate number of individuals falling into the parallel sections tallies pretty closely both for adults and for children (see Table I).[20] Here, therefore, lies a simple aim alike for educational administration and for voca-

[19] *The Distribution and Relations of Educational Abilities* (London County Council Reports, 1917). *Mental and Scholastic Tests* (London County Council Reports, 1922).

[20] Compiled partly from data published in the L.C.C. Reports (*loc. cit. sup.*), and partly from a table recently included in a paper on *The Principles of Vocational Guidance* (VIIth Int. Congr. Psych, 1923). The figures and categories given in the present table are round approximations only.

TABLE.

Distribution of Intelligence among Children and Adults.

Level of intelligence (in mental ratio)	Educational category or school	Number of children (in percentages)	Vocational category	Number of male adults (in percentages)
1. over 150 . . .	Scholarships (university honours)	0·2	Highest professional work	0·1
2. 130–150 . . .	Scholarships (secondary)	2·5	Lower professional work	3
3. 115–130 . . .	Central or higher elementary	13	Clerical, technical, and highly skilled work	12
4. 100–115 . . .	Ordinary elementary	35	Skilled work. Most minor commercial positions	27
5. 85–100 . . .	Ordinary elementary	35	Semi-skilled mechanical work. Poorest commercial positions	36
6. 70–85 . . .	Dull and backward classes	13	Unskilled labour and coarse manual work	18
7. 50–70 . . .	Special schools for the mentally defective	1·5	Casual labour	4
8. under 50 . .	Occupation-centres for the ineducable	0·2	Institutional cases (imbeciles and idiots)	0·2

A 4

tional guidance. It is the duty of the community, first, to ascertain what is the mental level of each individual child; then, to give him the education most appropriate to his level; and, lastly, before it leaves him, to guide him into the career for which his measure of intelligence has marked him out.

Of this programme, the educational part is already in execution. For the lowest section, the mentally deficient, we have begun to provide special schools and residential homes; and, thanks to the advances of individual psychology, the means of diagnosis are now exact and just. There is a similar but newer movement towards the institution of special classes for the dull and backward. It is from this larger horde of moderate dullards, not from the tiny sprinkling of the definitely defective, that the bulk of our inefficient adults—criminals, paupers, mendicants, and the great army of the unemployable—are ultimately derived. Nor will it do to confine official assistance solely to the inferior groups. The supernormal should also enjoy a special measure of care and treatment. Much is done for them by awarding free places at central and secondary schools. But both the methods for detecting them and the opportunities for educating them still admit of much improvement. Already in several foreign countries schools have been established for *Begabte Kinder*. In Berlin, the brightest children from the whole of the city are selected by means of psychological tests, and brought together at an early age to a special centre for individual supervision and training.

The determination of intelligence is equally indispensable for proper vocational guidance. Respecting intelligence, indeed, vocational psychologists seem unanimous that, as it is the easiest, so also it is the first and foremost factor to be tested. The worst misfits arise, not from forcing round pegs into square holes, but from placing large pegs in little holes, and small pegs in holes too big for them to fill. We have already seen that different occupational groups have different intellectual levels. For nearly every type of employment there exists a certain minimum of intelligence, below which a man is pretty sure to fail. For many, if not most, there is also, in all probability, an optimal upper limit. Just as some men are too dull for their jobs, so others are too clever. Hence, in the interests of the employer and of the employment, as well as of the employee and the general community, it is a blunder always to pick the brightest candidate who applies for a given job.

In this country, for the purposes of vocational selection, the most extensive application of intelligence-testing has been the introduction of a psychological 'group-test' into recent Civil Service examinations. The papers, comprising five or six graded speed-tests of well-known types, have been drawn up, after experimentation, by professional psychologists. Some 40,000 candidates have been tested in this way. And the calculated correlations demonstrate that the results of the new methods agree, both with the total marks from the whole examination and with subsequent reports on office-efficiency received from Government departments, more closely than any other single paper set.

Incidentally, the extensive data so secured ratify conclusions reached in other countries and by different means—namely, that the range of intelligence among adults is quite as wide as that observed among

children, that the average level of inborn intelligence among adults aged twenty to fifty is but little above that of children of fourteen, and that the distribution of intelligence, among adults as among children, approaches pretty closely to the so-called 'normal curve.' (See fig. 1, which shows the distribution of marks in the intelligence-tests set at the last Civil Service examination—Clerical Class, 1922.)

DISTRIBUTION OF INTELLIGENCE.

8599 ADULTS.

(Civil Service Examination—July 1922.)

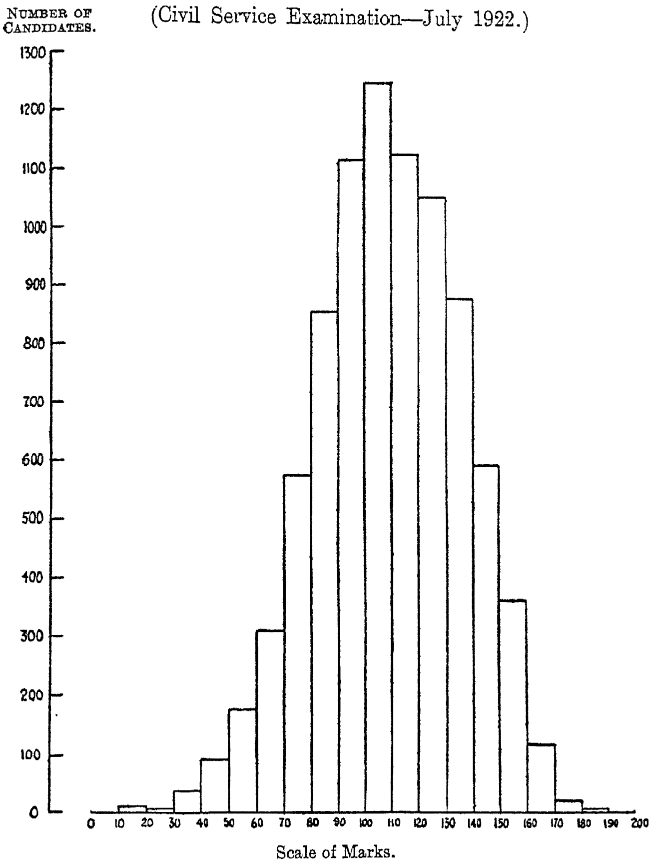

FIG. 1.

3. *Specific Abilities.*

With individual differences in general intelligence I have dealt at disproportionate length, partly because intelligence is, in Galton's phrase, a human quality of the utmost 'civic worth,' and partly because it is the one mental capacity upon which a prolonged and concentrated study has been focussed.

Over specific inborn abilities I need not linger. For them effective tests have proved disconcertingly hard to contrive. Simple correlation is here inapplicable. General intelligence is always getting in our way. We think we have tested something specific. We find we have only hit upon another test of intelligence. Its ubiquitous influence can only be eliminated by some elaborate technical device, the procedure, for example, known as multiple correlation; and the complexity of the whole task bewilders even where it does not baffle.

Nor do these special abilities, although presumably inborn, declare themselves at so young an age as the more general. Specialisation during the first twelve years of childhood is the exception rather than the rule. 'Young turtle,' says Epicurus, 'is every kind of meat in one—fish, fowl, pork, and venison; but old turtle is just plain turtle.' Similarly, the young child contains in fresh and dormant essence the germ of every faculty. Age alone betrays our idiosyncrasies. Adolescence is pre-eminently the period when many of these localised talents and specialised interests seem for the first time to mature. Accordingly, efforts at vocational guidance and educational specialisation must not be forced at too early a stage. At present, for example, the system of junior county scholarships tends to sweep all our brightest children at the age of ten or eleven into secondary schools of a somewhat academic type. When at a later period examinations are held for trade schools, most of the best instances of special talent are missing: they have already been creamed off and drafted into other directions less suited to their powers.

So far as it has been successful, the results of multiple correlation, eked out by other scattered indications, point to the following abilities as depending upon factors relatively specific: arithmetical, manual (drawing, writing, probably handwork of simpler kinds), verbal (reading and spelling), literary (composition in one's own tongue), linguistic (learning foreign languages), artistic, and musical, the last often appearing at an unusually early age. Of such specific or 'group' factors the specificity is not complete. There is much overlap; and, with every one of them, it is extremely hard to frame tests which depend mainly for success neither upon the 'central factor' of general intelligence, nor yet upon some particular capacity, so limited and local that no inference can be made from one performance to another, even within the same presumable group.

The abilities just enumerated seem undoubtedly specialised. But how far are they inborn? In practice what is actually tested must turn largely upon acquired dexterity, knowledge, and interest. And acquirements (as the classical experiments on formal training have taught us) tend always to be circumscribed; they do not diffuse or spread. The

old doctrine of native faculties is out of favour with the orthodox psychologist of to-day. We are told that there is no such thing as memory: there are only memories; that there is no such thing as a general power of muscular skill: there are only separate motor habits, each independently learnt. Nevertheless, the very way in which such acquirements are limited, particularly among individuals who have had identical opportunities at school and at home, argues an innate basis; and inquiries into heredity confirm the suspicion. On the existence and nature, therefore, of these hypothetical 'group-factors'—inborn powers that seem partly general but not entirely so, partly specific but not absolutely so—further research is imperatively needed. How far, for example, is there a group-factor underlying all kinds of memory, or all kinds of imagination, every form of mental quickness, every form of motor dexterity, and every form of apprehension through the several senses? Of the great difficulty of the problem, the prolonged work on mental imagery is an excellent example. The early experiments of Galton convinced contemporary psychologists that individuals might be classified into fairly definite types—the eye-minded, the ear-minded, the motor-minded, and so forth. That these sharp lines of demarcation can be no longer drawn has since been amply proved. But yet, in spite of countless inquiries, no satisfactory tests have been devised even for a capacity so clearly definable as visualisation; nor can we guess how far it may be specific, and how far it may be inborn, nor how far it is a manifestation of something more general, or how far it is simply a question-begging term for an aggregate of yet more limited habits or tendencies, each specific in itself.[21]

ii. *Acquired Attainments.*

I turn now from inborn abilities to those that are acquired. From a practical standpoint these may be broadly grouped into educational attainments and vocational attainments respectively.

For the teacher one of the most helpful achievements of experimental psychology has been the recent elaboration of standardised scholastic tests. Simple foot-rules have been scientifically constructed for measuring a child's knowledge of the chief school subjects—reading, spelling, arithmetic, handwriting, drawing, composition, and the like. By the help of such age-scales—those, for example, published by the London County Council—it is now practicable to assign, in the space of a few

[21] To hereditary differences of race, sex, and social class I have no space to allude. The main conclusion that can be drawn from experimental work is, I think, the following: Innate group-differences exist; but they are small. Training and tradition account for the more conspicuous. The inborn mental differences between class and class, between nation and nation, and between women and men, taken on the average and in the gross, are swamped by the far wider differences among the individual members that make up any single group. As to the mental differences between the two sexes—the topic upon which rather more experimental work has been done—the reader may be referred to the recent report of the Consultative Committee of the Board of Education on *Sex-differences and the Secondary School Curriculum* (H.M. Stationery Office, 1922).

minutes, his mental level for every branch of the elementary curriculum.[22]

To measure the effects of experience or training in a trade or business is almost as easy as to measure progress in school work. To determine the speed and accuracy with which a typist types, or a shorthand-writer takes down matter in shorthand, all that is needful is, first, to construct a simple test on scientific principles, and then to draw up, on the basis of actual experiment, standards of efficiency for work of differing difficulty. Tests for such acquirements are of use chiefly in vocational selection—where, that is to say, an employer desires to pick out for a given job the best in a list of applicants. Vocational guidance, on the other hand—where the adviser picks out for a given child the best of all possible jobs—is a far more intricate task. It demands the measurement, not of attainments, but of the underlying aptitudes. To test capacity is much harder than to test acquired knowledge or skill. This we have already seen. And to determine whether a child is endowed with sufficient intelligence, sufficient finger-dexterity, sufficient quickness in analysing sounds, for it to be worth while to train him as a shorthand-typist, is an infinitely harder affair than to discover whether, once his period of training is over, he has reached the minimum of practical skill that will be expected from an office clerk. Here then is yet another pressing problem for future experimental inquiry. The vocational psychologist must work backward from the measurement of acquired dexterities in every trade to the measurement of the related capacities. At present most tests that he administers hinge upon a blend of both. And, in spite of the theoretical difficulty of disentangling the two psychological components, the methods devised hitherto have already proved their value in factories, in workshops, and in commercial firms. In this country vocational tests have been drawn up, and are now being still further refined, not only for different kinds of clerical work, but also for dressmakers, miners, and the various branches of the engineering trades. The practical results, even in these early stages, are an unquestionable success.[23]

b. *Temperament.*

We have now reached the most delicate portion of every psychological analysis. Hitherto we have been studying the man's intelligence, of which he is not likely to be ashamed. Now we have to study his character, which he naturally prefers to keep private. Having seen the full-length portrait exhibited to public gaze, our ruthless hands must lift the picture from the wall, and turn it over, that our prying eyes may look upon the back.

[22] The teacher, unacquainted with the newer methods, will find the best introduction to the subject in Dr. Ballard's excellent and attractive little book, *Mental Tests.*

[23] Those desirous of further details may be referred to Professor Claparède's little pamphlet on *Problems and Methods of Vocational Guidance* (International Labour Office, Geneva, 1922); to Professor Muscio's *Review of the Literature on Vocational Guidance* (Reports of the Industrial Fatigue Research Board, No. 12, H.M. Stationery Office, 1921); and to articles and reviews in the *Journal of the National Institute of Industrial Psychology.*

Character has been defined as the sum-total of all those individual qualities which do not constitute, or are not pervaded by, intelligence; to avoid the specifically moral implications that cling to the popular word 'character,' I prefer to retain the old term 'temperament,' and use it in the sense defined. The qualities thus negatively grouped apart are not without a positive aspect shared by them all. Though they exhibit low correlations with intelligence, they yet show tolerably high correlations amongst themselves. Analytically, they are marked by affective and conative elements rather than by cognitive; by feeling rather than by knowledge; by will rather than skill.

Temperament or character is always more difficult to assess than intelligence. Intellectual qualities are relatively constant. Emotional qualities are evanescent and evasive—hard to seize, and harder still to measure. It is significant to note that, though the idea of temperamental testing is almost as old as that of intelligence-testing, it has seen quite a different career. Every one has heard of Binet's tests for intelligence. But most of us have forgotten his efforts to measure suggestibility, conscientiousness, and fidelity of report. Of late renewed endeavours have been made to test the feelings and the will; and of these the most effective are the methods of associative reaction and the so-called psycho-galvanic reflex. Pressey has tried to detect fears and repulsions by getting the examinee to pick out, from a prearranged list of words, those that have for him a special meaning, or suggest a special worry or dislike. Downey has tried to measure what she terms 'will-temperament' by seeing how far the candidate can modify at request his style of handwriting and manner of speech. Fernald measures self-control by the time the candidate can balance himself upon the ball of the foot. The Porteus mazes are to some extent a test of recklessness and impulsiveness. And the variability in repeated tests of almost any simple kind (as measured, for example, by the standard deviation) seems partly correlated with instability. But no tests of temperament can claim to have passed beyond the stage of tentative experiment.[24]

In assessing temperament, therefore, we must fall back upon the method of observation in place of the method of experiment. The personal interview is one recognised device; and another is the collation of reports submitted by competent observers who have been acquainted with the examinee during a long portion of his life. Both interviewing and reporting has each its own technique; and in either case the technique is susceptible of great improvement by the application of simple scientific principles. Much, indeed, has already been done by drawing up questionnaires of facts to be noted and observed,[25] and by

[24] A good summary of the literature, with a detailed bibliography, will be found in Cady's article on 'The Psychology and Pathology of Personality: A Summary of Test-problems,' *J. Delinq.*, vii., 225 (1922).

[25] Of these perhaps the most suggestive are those given by Webb, 'Character and Intelligence,' *Brit. J. Psych. Mon.*, I., and Hoch and Amsden 'Guide to the Descriptive Study of Personality,' *Rev. Neur. Psych.*, xi., 577. Cf. *Psych. Rev.*, xxi., 295.

contriving rating-scales[26] for the registration of such facts in terms of a comparable scheme.

i. *Inborn Emotional Qualities.*

As with intellectual qualities, so with emotional, it is both convenient and legitimate to distinguish at the outset the inborn from the acquired; and, so far as possible, to judge each level independently. In both directions much light has recently been thrown by the work of living authors. The inborn mechanisms have been tentatively catalogued and defined by McDougall; the acquired mechanisms by Freud and his school. The former lays stress upon hereditary factors; the latter upon developmental. But their views, however much opposed in general standpoint, are not so much incompatible as complementary. And they have this in common: both agree with one another in emphasising the dynamic elements in mental life, in contrast to the excessively intellectualistic preoccupation of the traditional psychology of the past. Each doctrine, although developed primarily as a correction of general psychological theory, is of the utmost practical value in studying the individual mind.

To sift and winnow inborn tendencies from those that are acquired is even harder in the realm of character than in the field of intellect. With adults it is all but impossible. With the young a few suggestions can at times be gleaned from the family history, or from the early personal history of the child himself. With children, too, the discrimination is more important practically. To know whether a spiteful boy is inherently ill-tempered, or only venting some half-hidden grievance; to know whether an erring girl is constitutionally oversexed, or merely putting into practice what she has picked up from corrupt companions; to separate the nervousness left by a shock from a chronic neurosis rooted in the system and likely to merge into madness or hysteria; to discriminate the excitability that is but a brief and transitory episode of some pubertal crisis from the excitability that began at birth and may last a lifetime—these are distinctions that make a world of difference in the treatment of the delinquent or neurotic while he is young.

α. *Specific Inborn Emotions and Instincts.*

English writers, McDougall, Shand, Drever, and others, find the foundations of human character in the instincts with their correlated emotions; and, taking their cue very largely from William James, they have given us useful working classifications for our common instinctive tendencies—inventories sufficiently identical for the purposes of the practical man. The strength with which each instinct is inherited is of necessity itself inborn. Accordingly, before estimating the character of a given individual, the first step is to take the universal human

[26] On rating persons either by 'relative position' or by reference to 'key-subjects' (a method elaborated with some success by the psychologists of the American Army) a rich literature has grown up. See, among other references, *The Personnel System of the U.S. Army*, vols. i. and ii.; Scott, *Psych. Bull.* xv. (1918); Thorndike, *J. Appl. Psych.*, ii. and iv. (1918 and 1920); and Rugg, *J. Educ. Psych.*, xii. and xiii. (1921 and 1922).

instincts one by one—pugnacity, fear, curiosity, disgust, sex, tenderness, gregariousness, and the like; and to ask in order with what intensity he has inherited each. In a study of juvenile crime I have endeavoured to show what an essential part the strength of the several instincts plays in determining the commoner forms of naughtiness and wrong behaviour in the young; in the elderly, and in the apparently virtuous, whether old or young, the same fundamental motives come more obscurely into play.

How can they be assessed? Not easily in the artificial and well-disciplined atmosphere of school or classroom; but with fair success, at any rate for delinquent and neurotic children, under more natural conditions where behaviour is spontaneous, as at home, in the street, in the playground, and in places of amusement generally. 'A man's nature,' says Bacon, 'is best perceived in privateness, for there is no affectation; in passion, for that putteth a man out of his precepts; and in a new case or experiment, for there custom leaveth him.'[27] The most serviceable method is to seek for certain standard situations, particularly those calculated to excite instinctive reactions; to observe the conduct of individual after individual; and so to gain by experience a notion of different grades of response. When relating to situations equally definite, the reports of parents, teachers, and the child himself provide suggestive supplements.

β. General Emotionality.

In a paper read some time ago before what was then the Psychological Sub-Section of this Association, I endeavoured to show that, in a random group, all emotional tendencies appeared to be correlated one with another in much the same way as intellectual. The child most prone to sorrow is often exceptionally prone to joy. The coward who bullies the weak is often the first to quake and quail before the strong. Correlations of this nature suggest the existence of a second central factor underlying the instincts and emotions, analogous to, but independent of, the factor termed intelligence. I have termed it 'general emotionality.' Those who manifest this inborn emotionality to an exceptional extent I call 'unstable'; and the most extreme cases 'temperamentally deficient.' And, in varying degrees, the existence of an unstable constitution is the chief characteristic feature of most delinquents and nearly all neurotics.

It is my view that a classification of the separate instincts, which shall be ultimately valid and convincing, can be reached only by the method of multiple correlation, by first eliminating, that is to say, the influence of the central factor, and then observing what specific factors remain, connecting particular forms of behaviour one with another.[28] If one makes a hierarchical table for the instincts and emotions, taken each as a unity, one seems to perceive the presence of a third set of factors—'group factors' of an intermediate level. When the influence

[27] *Essays*, xxxviii., 128.

[28] Only in this way can the issue between McDougall and Thorndike—whether the specific innate tendencies to behaviour are roughly six, or more nearly sixty or six hundred—be satisfactorily solved.

of the general factor has been eliminated, there emerge positive and negative correlations of a 'partial' order, which show that certain instincts tend to go more closely together than others. On the basis of such group-combinations we are led to distinguish certain broad emotional dispositions of at least two qualitatively differing kinds. On the one hand, the active or 'sthenic' emotions—anger, assertiveness, curiosity, joy, and perhaps sex—appear specifically correlated; on the other hand, the passive or 'asthenic' emotions—fear, submissiveness, disgust, sorrow, and perhaps gregariousness—seem in a similar way to be correlated with each other positively, but with the active or sthenic group negatively. Jung and his followers, working chiefly with abnormal patients, have recently thrown out some very suggestive speculations upon so-called emotional types. Their chief division consists in a revival and expansion of an old dichotomy. What have formerly been described as 'sensitive' and 'excitable' types, or 'restrained' and 'unrestrained' types, or 'subjective' and 'objective' types, or latterly 'herbivorous' and 'carnivorous' types, are now renamed 'introverts' and 'extroverts.' Once more, I believe the method of multiple correlation will afford the best way to confirm for the normal population these interesting deductions from pathology.[29]

ii. *Acquired Emotional Characteristics.*

Besides reviewing the strength of the several instincts and emotions which a man inherits, we must also investigate the more complex emotional tendencies that he has, in the course of his life-history, progressively acquired. These, according to the different angles from which they are regarded, and according to their own intrinsic nature, may be designated and sub-classified as habits, interests, sentiments, and complexes. We have, therefore, to inquire what habits each person has developed out of his instincts, what emotional attitudes he has unconsciously formed, what interests be has cultivated, and what ideals he has framed. These things are best ascertained through observation and interview. But the possibility of moral tests is already being investigated by the processes previously so successful in tests of intelligence. Attempts at measuring ethical discrimination, for example, have been made upon the following lines: a list of offences is drawn up, each described upon a separate card—breaking windows, scalding the cat, not going to church, stealing from a blind man's hat, flirting with a stranger, committing suicide, killing a thief, and the like; the examinee has to arrange them in order of wickedness. The arrangements of delinquents differ considerably from those of law-abiding children.[30] A

[29] I have no space to allude further to attempts to classify the basal psychopathic and neurotic types. I can only repeat that the trend of current work is to show that subnormalities in temperament and character, like subnormality in intellect, are extreme instances of milder deviations discoverable in the normal population. Useful references from the clinical standpoint are Wells, *Mental Regression: Its Concepts and Types*; Rosanoff, 'A Theory of Personality based on Psychological Experience.' *Psych. Bull.*, xvii., p. 281; and Paton, *Human Behaviour.*

[30] Fernald, *Amer. J. Insanity*, lxviii., 547; Haines, *Psychol. Rev.*, xxii., 303.

suggestive set of tests has been recently applied, by one American investigator, to a group of boy-scouts, and, by another, to groups of delinquent and non-delinquent children. The child is required to trace mazes with his eyes shut; to fill up and correct completion-tests with the key temptingly handy on the back; to state how much he knows of various topics, with the prospect of earning a box of confectionery if he obtains full marks. The measure is the number of times he cheats or overstates, and the results correlate with independent estimates of moral character to the extent of .42.[31] Sometimes (as in the last research) the examinee is also given a syllabus of questions relating to his own character: 'What kind of amusements do you prefer? Do you get on well with teachers and with other children? Would you like to wear jewellery and fine clothes? What do you think about when you are alone? What would you do if a lot of money were left you?' As a rule, however, an indirect technique is far preferable to a direct. The moral test is, as it were, to be camouflaged in the guise of a test of intelligence or information. The optional question-paper is full of possibilities in this direction. Every teacher knows how, in examinations on languages or mathematics, the routine worker chooses the mechanical questions, while the more enterprising select the problems and the riders; the cautious prefer the prepared texts; the adventurous the unseen translations. It is an interesting exercise to collect a set of picture postcards, artistic, humorous, or informative, and to request the child to arrange them in order of preference or merit. The influence of special interests, working quite unconsciously if the cards have been chosen with care, is nearly always obvious.

Few, however, would as yet pretend that such tests have more than an experimental interest. As Terman has put it: 'The reliability and validity of tests for moral traits have proved lower than an optimist might have hoped for. But the correlations obtained are quite as high as those yielded by the early intelligence tests of fifteen or twenty years ago. And this is no small achievement.'[32]

Conclusion.

Here, then, are the main items in the programme of the mental examiner. Here is my sketch of the skeleton of the mind.

Having tested all that he can test, having measured all measurable capacities, having passed in review all available data that throw light upon the rest, the psychologist must in the end bring his mixed materials together in one synoptic survey. He must reconstruct the mind dissected. The most expedient way of doing this is to plot out what is known in this country as a 'psychogram,' and elsewhere as a 'mental profile.' The various findings are to be charted diagrammatically upon some uniform and comprehensive scale. If he takes for his unit the percentile or the standard deviation, there is no capacity, no tendency,

[31] Voelker, 'The Functions of Ideals and Attitudes,' *Col. Univ. Contrib. Ed.* (1921); Cady, 'The Estimation of Juvenile Incorrigibility, *Journ. Delinq. Mon.* (1923).

[32] Preface to Cady's paper, *loc. cit. sup.*, p. 4.

no quality, in theory at any rate, that cannot thus be comparably expressed.

In his conclusions he will beware of four temptations. First, he must never court the applause of the unlearned, and the sneers of the worldly-wise, by claiming to have caught a living soul, and to have caged it in a formula, however technical, however abstruse. The growing mind is more than the sum of simple assignable elements; and all personal equations must issue in a surd. Similarly, he will avoid condensing his data at any point into vague generalisations—the announcement of a type, an average, or a total. A composite of snapshots, each taken from a different angle—a side-view, a full-view, a half-turn, and the rest—is no photograph at all; only an indecipherable blur. Thirdly, he must everywhere shun the besetting sin of the mere literary biographer—the confusing of facts with hypotheses to explain those facts, or, worse still, the submission of bare subjective inferences fortified by a string of anecdotes; data and interpretations the scientist keeps rigidly apart. Finally, throughout his inquiry, he must neither correct nor criticise, but coldly and calmly observe. His interest lies in realities, not in values; and should be 'positive,' as the philosophers say, not 'normative.' The teacher may psychologise while he is teaching, but he must not teach while he is testing. Nor should he anticipate the judgment-day by seeking to award praise or blame. His humble function is that of the recording angel, who registers, like a watch or a weighing machine, without audible comment.

There can be no denying that each inquiry will be slow, circuitous, and cumbersome. How long (it is sometimes asked) should it take to size up a single child? It was a tradition of the ancient world that no metamorphosis could hide a god from a god. And, upon a complementary principle, it seems often assumed that no disguise or taciturnity can save defectives or delinquents from the penetration of the mental expert. He is expected to cast his eye round the classroom or the prison, and to make a darting snapshot diagnosis on the spot. Our school doctors are given about ten minutes to decide whether a boy is deficient or not. Our magistrates take fifteen or twenty to determine what is best for a first offender. But the laboratory tester thinks himself a miracle of swiftness if he has measured a child's intelligence in less than an hour; and the psychoanalyst asks his startled patient for six months of separate weekly sittings to unravel a single complex. A longer period still was required by Shakespeare:

> 'It is not a year or two shows us a man.'

And Dr. Johnson thought the intimacy of a lifetime scarcely enough: 'God Himself, sir, does not propose to judge man until the end of his days.' Whether they be normal or subnormal, backward, delinquent, or neurotic, or merely youthful applicants seeking their most appropriate career in after-life, we can deal with human beings fairly and efficiently only by making an intensive, individual study of each isolated mind; there is no other way. Human personality, with all its infinite variety, is the most important single factor in all our social life; and the expenditure of time, however lavish, will never be lost.

Where the exigencies of the case demand a speedy assessment, I recommend the practical psychologist to aim chiefly at the so-called general factors. If I were permitted to measure no more than a pair of mental qualities, I should look first to the degree of a man's native intelligence—his 'general ability,' with which more special capacities are known to be correlated; and next to the degree of his native instability—his 'general emotionality,' with which his special instincts are apt to be in accord. Were I granted the grace of two or three additional estimates, they would still be of a general type—general physical health, general moral character, and general cultural attainments.

It may be that I am too optimistic, and that my views are premature. But it is my personal conviction that the main outlines of our human nature are now approximately known, and that the whole territory of individual psychology has, by one worker or another, been completely covered in the large. We have viewed the whole continent from above by rapid aerial flights towards different quarters. It remains to link up and to co-ordinate the numerous reconnoitring pioneers; then to descend, and, by the laborious method of exploring feature after feature, to correct up our maps in definite detail. Once its broad principles have been determined, it is from the close and microscopic detection of minutiæ, of tiny items and small but telling indications, that every science is eventually built up. This must be the aim with individual psychology in the near future. We must discover what mental traits are relatively independent, and which are the general among the relatively specific; we must construct precise working definitions for each, and hammer out by experiment upon experiment, research upon research, tests and rating-scales for everything that can be quantitatively expressed, inventing new tests for traits not hitherto tested, and refining the procedure of the old. Here rather than in any grand discovery must further progress lie.

Finally, let me leave the would-be analyst of character with a repetition of a warning already uttered in another place. Individual psychology is not a code of rules and principles to be mastered out of hand in the lecture-room or laboratory. It is not an affair of text-book terminology or of a teachable technique. It is the product of worldly experience acting on an inborn interest—an enthusiasm for persons as persons, in the old *nihil alienum* spirit. To take an unknown mind as it is, and delicately one by one to learn its chords and stops, to 'pluck the heart out of its mystery, and sound it from its lowest note to the top of its compass,' is an art and not a science. The scientist may standardise the methods. To apply those methods, and appraise the results, demands the tact, the temperament, the sympathetic insight of the genuine lover of strange souls.

SOME ASPECTS OF THE PRESENT POSITION OF BOTANY.

ADDRESS BY

A. G. TANSLEY, M.A., F.R.S.,

PRESIDENT OF THE SECTION.

We meet to-day in a city which is one of the greatest seaports of the kingdom, traditionally the main channel of our commerce and intercourse with the great English-speaking republic across the Atlantic, and also the main centre of the import of cotton and of the export of cotton goods, with which the prosperity of Lancashire, and to no small degree of the country, is so intimately associated. To the enterprise and public spirit of the citizens of Liverpool we owe the creation and development, within an astonishingly short period, of the distinguished university the hospitality of whose staffs, organisations, and buildings we shall enjoy during the coming week. Many of us can vividly remember the pride and satisfaction with which we saw arise, especially during the last decade of last century, one after another of the great institutes of research and teaching which have contributed so much to the advancement of science in the comparatively few years during which they have existed. In such surroundings we cannot but be stimulated afresh to labour to the limit of our abilities in the cause of that great human activity—the advancement of science in all its branches—which as members of the British Association we all have at heart.

Since the last meeting of the Association we botanists have to mourn the loss of two striking and dominant personalities. Sir Isaac Bayley Balfour played a great and worthy part in that revival of scientific botany in this country which marked the last quarter of last century. During his long tenure of the Chair of Botany at Edinburgh and of the Directorship of the famous Botanic Garden in that city, he was widely known for the ability and assiduity with which he carried out the work of one of the most important and onerous botanical positions in the kingdom, and for the native shrewdness and sanity, the ripe judgment and experience, which he was always ready to place at the disposal of his colleagues. Mr. Henry Elwes was a country gentleman of a type for which England has long been famous, who, like Lord Herbert of Cherbury, conceived it 'a fine study and worthy a gentleman to be a good botanique that so he may know the nature of all herbs and plants, being our fellow creatures.' To this study Mr. Elwes brought the utmost energy and vigour, pursuing to the remotest lands, both personally and by deputy, an untiring search for the objects of his attachment. He

will best be remembered by that magnificent work 'The Trees of Great Britain and Ireland,' which, in conjunction with Professor Augustine Henry, he produced at his own expense on a splendid scale.

I propose to deal this morning with some aspects of the development of pure botany during the last thirty or forty years, especially in this country, and with the bearing of these developments on the present position of the subject. In seeking for a suitable starting-point from which to begin the observations I have to make I naturally turned to the address delivered by my predecessor in this chair at the last meeting of the Association in this city. On that occasion, in 1896, the chair of Section K was occupied by Dr. D. H. Scott, and I found at once that the remarks with which he began his Presidential Address were surprisingly apt for my purpose. For definiteness of outlook on the problems of pure botany and for lucidity of expression they could not be surpassed, and their author will, I am sure, forgive me if I use his statement as the primary text from which to develop my critical exposition.

'The object of modern morphological botany,' said Dr. Scott, 'is the accurate comparison of plants, both living and extinct, with the object of tracing their real relationships with one another, and thus of ultimately constructing a genealogical tree of the vegetable kingdom. The problem is thus a purely historical one, and is perfectly distinct from any of the questions with which physiology has to do.

'Yet there is a close relation between these two branches of biology, at any rate to those who maintain the Darwinian position. For from that point of view we see that all the characters which the morphologist has to compare are, or have been, adaptive. Hence it is impossible for the morphologist to ignore the functions of those organs of which he is studying the homologies. To those who accept the origin of species by variation and natural selection there are no such things as morphological characters pure and simple. There are not two distinct categories of characters—a morphological and a physiological category—for all characters alike are physiological.' And then the President proceeded to quote, evidently with full agreement, from Professor (now Sir) Ray Lankester. 'According to that theory' [*i.e.* the Darwinian theory], wrote Professor Lankester in 'The Advancement of Science,' 'every organ, every part, colour, and peculiarity of an organism must either be of benefit to an organism itself, or have been so to its ancestors. . . . Necessarily, according to the theory of natural selection, structures either are present because they are selected as useful, or because they are still inherited from ancestors to whom they were useful, though no longer useful to the existing representatives of those ancestors.' And a little further on Dr. Scott said: 'Although there is no essential difference between adaptive and morphological characters, there is a great difference in the morphologist's and the physiologist's way of looking at them. The physiologist is interested in the question how organs work; the morphologist asks, What is their history?'[1]

The way of looking at the science of biology so clearly expressed

[1] *British Association Report,* Liverpool Meeting, 1896, pp. 992, 993.

in these sentences was by no means exceptional. Indeed, it may be fairly called the orthodox view at that time. Thus five years earlier, in 1891, Professor Strasburger, perhaps the most brilliant and successful German botanist of what we must now speak of as the last generation, wrote in the preface to his great work on the structure and functions of the conducting tissues: 'Morphology as such is a purely formal science, and thus corresponds approximately with comparative grammar, in that it explains forms by deriving them. It need be as little influenced by the functions of the forms to be derived as comparative grammar is influenced by the meanings of words. Not that a physiological treatment of the external and internal structure of a natural body would be less fruitful than the morphological, but it forms a different discipline.' After referring to the unfortunate effect of physiological points of view on the work of the earlier anatomists, who called, for instance, the water-conducting elements of plants 'tracheal' because they thought they were air passages, Professor Strasburger proceeded: 'With advancing enlightenment the provinces of morphology and physiology were separated from one another and developed on separate lines, without, of course, attaining complete independence . . . in fact, organs and functions are not separated in nature, and are only logically distinguished in order to subserve the building up of science. . . . Morphology finds its task only in deriving one form from another, in tracing different forms to a common origin. When this is successful the goal is reached. . . . The way which leads to morphological understanding is that of comparison, but only because this way involves a phylogenetic significance. Since a direct phylogenetic proof of the origin of a given structure is not to be had, morphology remains tied to indirect methods. It is often supported in its task by ontogeny, but only in so far as this is capable of giving phylogenetic points of view.'[2] Here we have the same insistence on the separateness of the two disciplines, morphology and physiology, and the same clear statement that the object of morphology is the elucidation of phylogeny. We may note, however, one striking difference. Professor Strasburger thinks that morphology need be as little influenced by the functions of the forms to be derived as comparative grammar by the meanings of words, and he does not claim, like Dr. Scott, that all features of an organism are, or have been in the past, adaptive.

It is, I think, impossible to regard the views thus expressed by a representative English and a representative German botanist three decades ago as representing to-day an adequate outlook on the problems of botany as a whole; and I shall be engaged this morning in endeavouring to expound the view which I think we should put in its place. First, I must pay some attention to the causes of the orthodox attitude of the last generation, the generation in which I was botanically brought up, and whose orientation I fear I passively accepted. The main cause of the greatly intensified interest in comparative morphology which led to the claim that this subject represented a separate discipline, co-

[2] E. Strasburger, *Ueber den Bau und die Verrichtungen der Leitungsbahnen in den Pflanzen*, Histologische Beiträge III, Jena, 1891, p. vi.

ordinate with physiology, was, of course, the general acceptance by biologists of the doctrine of descent with modification, popularly called evolution. Belief in the reality of this process at once invested the comparative study of structure with a new fascination. Every part, every organ of an animal or plant, could be interpreted in the light of the doctrine of descent. All the species of a group should, according to the theory of descent, be theoretically traceable to a hypothetical 'common ancestor' of the group, and these group ancestors again to remoter ancestors. Ultimately we should be able, theoretically at least, to reconstruct the whole genealogical tree of the plant and animal kingdoms. It was, of course, recognised that we could never hope to complete this task, even if we possessed an exhaustive knowledge of the structure and development of every kind of organism now living, for very many forms had been destroyed and had disappeared altogether in the course of the evolution of the organic world as it exists to-day. But the remains of many of the organisms which had lived in past ages were still preserved as fossils, and a knowledge of their structure would substantially help us on the way to the goal, even though that goal could never actually be reached. Though the geological record was extremely fragmentary, yet it did bring to our knowledge many kinds of plants, some more or less closely allied to living forms, others which could not be placed in any living group, and others, again, which suggested that they might represent or at least stand near to the common ancestors of existing groups.

If we consider the most recent developments of the subject we find that, on the whole, the search for common ancestors as such has been disappointing. The 'seed-bearing ferns' (Pteridosperms) have turned out to be, so far as we can tell, a perfectly independent group having no demonstrable connection with the true ferns. The most primitive fossil ferns known (the Primofilices or Cœnopterideæ of the Lower Carboniferous) certainly represent a very ancient group. But not only, according to Dr. Scott in his most recent statement,[3] do 'Pteridosperms and Ferns at all times show themselves perfectly distinct': 'we are dealing, in the Lower Carboniferous Primofilices, with early races already specialised on their own lines, and probably only indirectly connected with the main current of Fern-evolution.'

The remarkable Rhynie fossils described by Kidston and Lang from the Lower Devonian—the oldest vascular plants with structure preserved that are as yet known—have revealed in the genera *Rhynia* and *Hornea* a leafless and rootless type with large simple terminal sporangia and a simple stele occupying the centre of the axis. These plants show striking points of agreement with the living Psilotales, but their discoverers, so far from being prepared to assert that they are prototypes of Psilotales, create for their fossils a new class, the Psilophytales. Thus we have now recognised six distinct classes or orders of living and fossil Pteridophytes,[4] and parallel with these six distinct classes of non-

[3] D. H. Scott, 'The Early History of the Land Flora,' *Nature*, Nov. 11, 1922.

[4] Psilophytales, Psilotales, Sphenophyllales, Equisetales, Lycopodiales, Filicales.

angiospermous seed-plants, two wholly and others largely fossil.[5] In addition there are still a multitude of fossil forms, largely detached fragments such as sori, seeds, leaves, or wood, which are not sufficiently known or correlated to permit of their definite assignment to one or other of these classes. From these and for other discoveries it may well turn out to be necessary in the future to construct one or more new classes.

Leaving these great series of vascular forms which played so dominant a part in the history of vegetation during the Primary and Secondary geological epochs, we may note that the gulf which has always existed for the phylogenist between Pteridophyta and Bryophyta is as wide and deep as ever, and that the same may be said of the gulf between the Bryophyta and the Algæ. The attempts which have been made from time to time to derive various groups of Fungi from various groups of Algæ seem to me quite unconvincing. The phylogeny of the Fungi themselves remains obscure, though certain lines of advance among them and among the Algæ are fairly probable. On the whole the most successful phylogenetic speculations seem to me to be those that trace some at least of the classes of Algæ back to a common origin in the great plexus of the Flagellata, which may also, perhaps, be regarded as the likeliest recognisable starting-point of the main lines of invertebrate evolution. Turning to the other extremity of the Plant Kingdom, to the characteristically modern dominant vegetation of the earth, we are scarcely able to form a trustworthy opinion as to the nature of the plants from which the two great modern groups of Angiosperms sprang, though the speculations of one of my predecessors in this chair, the late Miss Sargant, founded on wide researches and elaborated with masterly ability, are certainly of great interest, and full of suggestion as to what may have occurred. The evidence from fossil Angiosperms is still unsatisfactory, and Mr. Hamshaw Thomas's interesting discoveries of Jurassic Angiosperms scarcely throw light on the problem of the descent of the group. It has been the invariable history of such researches, pursued with a view to tracing phylogeny, that the better a newly discovered group has become known the less probably it appears to represent the common ancestors of other existing or fossil groups. The points of origin, the roots, so to speak, of each group have been constantly lengthened and shifted further back in geological time so that they become more definitely independent from one another and appear to issue separately from a past which remains obstinately obscure. 'It may be,' said Professor Seward recently,[6] speaking from the fullness of a very wide knowledge of the floras of the past, 'that we shall never piece together the links in the chain of life, not because the missing parts elude our search, but because the unfolding of terrestrial life in all its phases cannot be compared to a single chain. Continuity in some degree there must have been, but it is conceivable that plant life viewed

[5] Pteridospermeæ, Cordaitales, Cycadophyta, Coniferales, Ginkgoales, and Gnetales: see Seward, Fossil Plants, vols. III. and IV.

[6] A. C. Seward, 'A Study in Contrasts,' (Hooker Lecture), *Journ. Linn. Soc Bot.*, October 1922, p. 238.

as a whole may best be represented by separate and independent lines of evolution or disconnected chains which were never united, each being initiated by some revolution in the organic world.' And again,[7] the development of vegetation 'appears as a series of separate lines, some stretching into a remote past, others of more recent origin.' 'It would almost seem that "missing links" have never existed.' 'There is no insuperable objection to the conception that terrestrial vegetation received additions from upraised portions of the earth's crust at more than one epoch in the history of the earth.' The picture of the history of evolution here suggested makes the search for common ancestors literally a hopeless quest, the genealogical tree an illusory vision.

But there can be no doubt whatever that the great body of work originally stimulated and inspired by the ideal of the genealogical tree has added very greatly to our knowledge of the range of the forms and structures of plants, notably of vascular plants, and of the rise and fall of the great groups during the passage of geological time. In regard to the structures of plants, it has directed attention especially to the vascular system of the plants of the middle grades of organisation, and given us a much more extensive and accurate acquaintance with the larger features of its organisation and development throughout these grades. We have discovered that vascular structure shows a type of progression from simpler to more complex forms which is broadly identical along many different lines of descent, a progression closely paralleled in the ontogeny of the individuals belonging to species which exhibit the more complex adult structure; and we have thus learned to correct the one-sided emphasis that used to be placed on the reproductive organs as guides to evolution. Though these last remain, so far as we can tell, the most trustworthy indices of *affinity*, yet 'the characters of the vascular system,' says Professor Bower in his recently published book on the Ferns, are 'the most important structural features for the phyletic treatment of the Class.'[8]

Without question, then, morphological and paleobotanical work, particularly in its extension to the internal structure of plants, has added greatly to our knowledge of the plant kingdom, and has given us a much fuller and juster appreciation of the range of the great middle groups, and to some extent of their relationship, or, as perhaps we should say, of their lack of relationship, to one another. One of the most striking results of this work as a whole has been the increasing doubt it has engendered as to whether many organs formerly regarded as homologous in the strict sense, *i.e.* homogenetic, of common origin in descent, are really homologous in this sense at all. The principle of homoplastic or parallel evolution has been more and more widely extended. And our increasing though still very rudimentary knowledge of the factors which determine organic form would suggest not only that parallel evolution has been determined by parallel conditions of life, an idea long familiar to biologists, but that we should expect a recurrence of the same formative factors, producing similar structures, on different lines

[7] Presidential Address to the Geological Society, 1923.

[8] F. O. Bower, *The Ferns (Filicales)*, 1923, vol. I, p. 192.

of descent, and to a large extent independently of particular life conditions.

It seems to me that no structure which has been assumed to be homologous throughout a large series showing many gaps is really safe from the suspicion of having been developed independently on different lines of descent. In a recent paper Dr. Scott writes [9] of the inference 'that the Seed Plants, of which the Pteridosperms are among the earlier representatives, constitute an independent phylum, of equal antiquity with any of the recognised lines of Vascular Cryptogams.' But is it at all certain that the Seed Plants really constitute a *single* phylum? Is it not perfectly possible that the seed with its attendant mechanisms has been independently evolved in some or all of the six classes of Seed Plants which, apart from the Angiosperms, are now recognised? It is clear that the more such suspicions effect permanent lodgment in our minds the more uncertain all wide positive phylogenetic conclusions must become.

Meanwhile the whole of this branch of botany seems to leave the great majority of the younger botanists cold. No longer under the immediate influence of the revolution in biological ways of thinking brought about by Darwin, they are not greatly interested in comparative morphology, nor in the attempts to disentangle the past history of the plant kingdom, sustained and even magnificent as these attempts have been, and greatly as they have enriched our knowledge of the past life of our world. There is, to many of them, an effect of hopelessness and even of futility in the effort to trace out the course of the threads in an intricately woven carpet, with no attainable certainty that we have got them right, however long and patiently the task is pursued, partly because so many of the threads, such large portions of the carpet, have been destroyed for ever, partly because, as Professor Seward suggests, we may, in effect, be dealing not with one carpet, but with many.

While we may urge that far too much time has been, and in many places still is, devoted to the study of comparative morphology in elementary teaching, it is impossible to deny the great interest and importance of conclusions like those quoted from Dr. Scott and Professor Seward. From the point of view of the ideal of the 'genealogical tree' these conclusions are negative, but they are none the less interesting and valuable, for they are giving us a truer view of the past history of plants.

There has certainly been no loss of interest in the *process* of development, whether phylogenetic or ontogenetic. The unfolding of life upon the earth, the marvellous story of development and change, of increasing complication and endless variation on the one hand; and on the other the great problem of how the complex organism comes to develop from the minute zygote, can never lose their fascination for the human mind. It is the formal comparison of the end results of this process, with a view to the determination of phylogenetic relationships, the treatment of the problem as 'a purely historical one,' which seems to so many of the keenest younger biologists a hopeless and not a very

[9] *The Origin of the Seed Plants,* p. 227.

remunerative pursuit. Their interest is in the process itself rather than in the phylogenetic connexions of its particular results. They want to know what brings about development and evolution, what are the driving forces behind these processes.

The orthodox 'Darwinian' answer to this question, so far as it applies to phylogenesis, was 'natural selection.' The organism was supposed to be capable of indefinite 'spontaneous' but heritable variation in all directions and of various degrees, and those which happened to be useful to the organism by giving it a decisive advantage in the struggle for existence were preserved because the individuals which showed them alone survived and produced offspring, which inherited the useful variations and thus modified the species. Two or more divergent sets of variations might happen to fit different individuals of a parent species to different sets of conditions, different habitats, into which they had wandered, while the parent species remained behind unmodified in the original habitat, and thus new varieties were supposed to originate. By the further development of the new characters, *i.e.* by the favouring of further variations in the same direction as the original ones, these varieties became distinct species. The same process further continued and involving also other structural features would lead to the wider divergence of the derivatives from the original stock, and this divergence would ultimately become so great that the different forms would be placed in distinct genera. The sharpness of the specific and generic distinctions would often be enhanced by the disappearance of the original or of intermediate forms, owing for instance to the physical conditions of life changing and becoming unsuitable for them or to their suppression by rivals whose variations had been more successful. In the course of a very long time, by a continuation of the same processes, the distinctions which were at first specific, and later generic, would become family distinctions, later again ordinal distinctions, and so on up to the great phyla.

Alongside of the evolution of new species, genera, and families in the same general environment, such for instance as the tidal zone, there had been a migration of some forms to the land, or perhaps, as Mr. Church would have us suppose, a gradual raising of the land bearing aquatic forms above the water-level. These aquatic forms had thus been faced by conditions of life very different from the earlier ones, so that the variations which were preserved and perpetuated were necessarily in new directions and gradually built up the equipment of the land plant —the typical leaf and root, the vascular and aerating systems, the cuticle, the air-distributed spores. From these earlier land plants again by further variation the heterosporous forms were derived, and finally the seed and angiospermy, while various progressive complications and modifications of the primitive vascular tissue, including secondary thickening, had established both more copious and more efficient conducting and mechanical systems, and thus led to the quickly growing, largely upright, modern plants, extraordinarily 'flexible' to various life conditions.

I think this is a fair rough statement of what was often known as the Neo-Darwinian account of evolution, as applied to plants, in the

last decade of last century. It was not precisely Darwin's own position, but gained its great vogue, especially in this country, largely through the writing of Alfred Russel Wallace, and through the germ-plasm theory of August Weismann. All characters whatever, as Dr. Scott and Sir Ray Lankester said, were regarded as adaptive or useful in the first instance, and as produced by the summation of small variations. The origin of these variations was obscure. The fact that such variations occurred was sufficiently established, and their occurrence was simply taken as a datum on which natural selection could work by picking out and establishing the favourable ones. The only characters which were not considered adaptive at their first origin were covered by the conception of 'correlated variation,' *i.e.* structural or functional changes necessarily involved by the primary adaptive ones, though not in themselves useful to the organism. Later on the structural changes which were at first useful might be so no longer, owing to their supersession by other structures or by a change of conditions. They were, however, or might be, still inherited, being incorporated in the constitution of the organism's germ-plasm, though superseded, so far as current adaptation went, by more recently acquired characters, as in the familiar case of the embryonic gill-slits of the higher vertebrates. Frequently an organ originally acquired for one purpose was diverted to different uses, as for instance the anterior fins of fishes, which became, in their modified terrestrial descendants, legs, arms, or wings. Thus the actual structure of an organism could only be explained by its ancestral history.

The weak point of this theory of evolution, on the facts then known, apart from the obscurity surrounding the origin of variations, was the difficulty of understanding how the first minimal variations, which were supposed to be the foundation of new structures, could, at least in many cases, be of life-preserving value—' survival value,' as the phrase goes—to the organism, and how they could avoid being 'swamped,' as it was supposed would happen, by intercrossing with other unmodified members of the species. Various theories of segregation, geographical or physiological, were proposed to get over this difficulty, but it was very doubtful if they could be considered as of sufficiently wide application for the purpose. Further, the theory required that the actual structural differences between species—apart from 'correlated variations'—should *always* be adaptive; yet the greater number of naturalists who had a wide first-hand acquaintance with species as they exist in the field, and with the actual differences between allied species, could not find that this was the case. Some people attributed this scepticism to ignorance of the functions of particular structures which seemed to be useless, the Neo-Darwinians refusing to admit that constant characters might have no 'function' after all, unless they were vestigial or 'correlated' with others that had. The field naturalists, however, remained for the most part obdurate. One distinguished biologist, referring to the hope that all specific characters would ultimately be proved adaptive, added, 'Time has been running now and the hope is unfulfilled.' Ingenious persons explained all sorts of peculiar structures and arrangements—'myrmecophily,' the insectivorous habit of some plants, extra-floral nectaries, the long tips of certain tropical leaves, and countless others—

as of use to the species that exhibited them, always with the implication, and sometimes with the express assertion, that they had been developed *because* of their survival values. One by one, in the light of critical research, most of these 'explanations' of structure have broken down. Not only is 'survival value' almost impossible to prove in any given case, but many of these supposed adaptive structures or arrangements have been shown not to work in the way they were supposed to work. Nevertheless, the habit has remained, even up to this day, not only of looking for the 'use' or 'function' of every structural character—quite a legitimate proceeding in itself, if we are not wedded to the belief that it *must have* a 'use'—but of considering its existence sufficiently 'explained' when such a use has been experimentally established or even more or less plausibly suggested.

Round about the beginning of the present century several publications of first-rate importance began to put a new complexion on these problems. First there was De Vries's work on mutations,[10] which claimed to show that discontinuous variation, whose widespread occurrence in nature had already been demonstrated by Bateson and suggested by him as the prime cause of the discontinuity of species,[11] was *the* important factor in evolution. In 1903 Johannsen's work on 'pure lines'[12] showed in the most unmistakable manner in the case of the bean that the minimal 'fluctuating' variations, on which Wallace and the Neo-Darwinians had been accustomed to rely as the material on which natural selection operates, are *not* inherited, so that if one breeds from a group of such variations which deviate from the mean of the pure line, there is no establishment of a deviating mean in the descendants, but a regression to the original mean. Meanwhile, the rediscovery of the Mendelian phenomena and the rapid extension of the range of characters in which they were found to be exhibited had at last placed our knowledge of the mechanism of heredity and variation on a secure basis. The immense quantity of breeding and cytological work which has followed has given reality to the conception of 'genetic constitution,' or *genotype* as it is called in current terminology. We now know that an ordinary 'Linnean' species is, often at least, an aggregate or mixture of crosses from 'pure lines' in respect of different characters, each pure line with a specific genetic constitution based on the structure of the chromosome complex. New *heritable* variations of the stock are produced by redistribution of units within the chromosomes resulting from the crossing of individuals belonging to different pure lines or of their hybrid offspring. This apparently occurs in the stage of 'synapsis' of the nuclei which are just entering upon the divisions that result in the tetrads of spores and gametes; and it is followed by the 'reduction division' of the mother cell of the tetrad, resulting in segregation of unlike units so that the gametes of a single tetrad bear different characters. Other internal changes in the chromosome complex may perhaps take place, but of these we can as yet say very little.

[10] Published in a series of papers culminating in his great work, *Die Mutationstheorie*. Leipzig, 1901. Vol. II, 1903.

[11] Bateson, *Materials for the Study of Variations*. London, 1894.

[12] Johannsen, *Ueber Erblichkeit in Populationen und reinen Linien*. Jena, 0

It is to be noted that these great discoveries do not *necessarily* invalidate the Neo-Darwinian position. It is still perhaps just possible to hold, so far as this new knowledge of the mechanism of variation and heredity is concerned, that in any given complex of forms which we call a species, only those variations are in the long run preserved which adapt the individuals that show them more closely to their conditions of life. But the more exact knowledge we now possess of the way in which new heritable variations in the body of the organism actually come to arise and maintain themselves has firmly established the thesis, clearly stated by Bateson nearly thirty years ago[13], that the primary problem of evolution is the process of variation itself and not what happens to the variations in the struggle for life after they have appeared. Variations from type, more or less considerable, actually arise by new combinations of the primary chromosomal determinants—*genes* as they are now called, by the loss (dropping out) of certain genes, or perhaps by actual changes in the nature of the genes or the appearance of new ones; and the variations so produced persist, or may persist, indefinitely, without any reference to selection. It is perfectly true, of course, and must always remain true, that every organism which survives must be viable and *sufficiently* adapted to the conditions of its existence. But it is not only unproved, it is a gratuitous belief unsupported by the evidence, that all new characters, all differences between species, are of survival value or owe their origin in any way to selection.

The clearest and most plausible account of the origin of new species, in the light of our existing knowledge, is, it seems to me, that given by the Hagedoorns.[14] Any group of related individuals capable of interbreeding, so far as its somatic characters are genetically determined, owes those characters (*phenotype*) to the totality of the genes possessed by the zygotes from which they were produced (*genotype*). Some of the genes present may not, however, affect the phenotype because they do not meet with the developmental or environmental conditions necessary to enable them to find expression in the soma, or because some other gene or genes, interaction with which is necessary to phenotypic expression, may be absent. The total actual 'genetic' variability of the group is measured by the total range of phenotypic variability: the total *potential* variability is greater than this because it includes the potential effects of the genes which are present, but which may, at any given moment, be inoperative for the reasons cited. The total potential variability is measured by the number of genes for which the group is not pure. If the whole group is pure, uniform and homozygous for all characters, it cannot, by hypothesis, vary genetically. The potential variability of the group is increased if there are taken up into it individuals which either possess a gene or genes not present in any member of the group or which lack genes that are the common property of all the original members of the group. Thus if fresh crossing takes place with fertile individuals outside the group, the potential variability of the original

[13] W. Bateson, *op. cit.*, p. 6.

[14] A. L. and A. C. Hagedoorn, *The Relative Value of the Processes causing Evolution*. The Hague, 1921.

group is increased. But in the absence of this, in fact in all cases where the group is isolated, mechanically or otherwise, the potential variability constantly tends to *decrease*, because the offspring of any generation are normally produced from a small fraction only of the individuals of that generation, and this leads to the dropping out from the breeding stock of part of the total potential group variability. In the case of a self-fertilised plant the reduction of variability will proceed even if all the individuals produce offspring, because Mendelian segregation will result in the daughter being heterozygous for only one-half the number of genes for which the mother was impure. In the absence of crossing with individuals having a different genotype, heterozygotes will produce some homozygotes, but homozygotes can never produce heterozygotes, so that the proportion of heterozygotes in such an exclusively self-fertilised race will steadily decrease. This is an intelligible view of the origin of the discontinuity of species. The mechanism will work whether natural selection is in play or not.

Suppose, for instance, that from a breeding stock with a given total potential variability a number of islands are colonised. The colony on each island will, on random selection, have a substantially smaller total variability than the original stock, because it will be derived from a much smaller number of individuals, and a good part of the original variability will be lost. Further, if the colonising groups are selected haphazard the potential variability of each colony will be different, and the offspring of the different colonies will form as many new 'species,' each of which will in successive generations increase in purity. The differences between these species may, however, have no relation whatever to adaptation, because the characters in which the new species differ from one another and from the parent species may have no survival value in any of the habitats. Many years ago J. T. Gulick called attention to the fact that the species of land molluscs on the Sandwich Islands showed differences which did not seem to be adaptive, but which were closely related to isolation.[15] More recently Crampton has arrived at similar results in regard to the forms of another land snail, *Partula*, and has actually shown that numerous new forms have arisen, as he holds by mutation and isolation, since the distribution of the forms was accurately recorded in 1884.[16] On the other hand, if the new habitats differ, and there is variability in the original genotype corresponding with phenotypic characters which have survival value in relation to the differences of habitat, selection will play its part in determining the genotypes of the new species. Thus we can understand why it is that geographically isolated but clearly allied species *may or may not* differ in 'adaptive' characters. We can also understand how it is that different closely allied species come to exist in the same geographical area but in different habitats—different 'ecological niches' as they have been called—between which the chances of crossing are at a minimum, *either with or without* specific adaptation to the habitat. It depends

[15] J. T. Gulick, 'Divergent Evolution through Cumulative Segregation,' *Journ. Linn. Soc. Zool.*, 20, 1888.

[16] H. E. Crampton, *Studies on the Variation, Distribution and Evolution of the genus* Partula. Carn. Inst. of Washington, 1917.

upon whether the random selection of individuals which originally colonised these habitats varied from the genotype of the parent stock in characters of survival value in relation to those habitats or not. Where two such habitats abut on one another and there is no specific adaptation to the two habitats intermediates of hybrid origin are often found along the line of contact.

In plants which are self-fertilised as a rule, but in which crossing is not absolutely excluded, numerous species may come to exist in the same geographical area and the same habitat, for the changes in genotype brought about by the occasional crossing will be fixed and the phenotype purified, *i.e.* rendered more homogeneous, by the subsequent isolation for many generations of the different families. It is in this way that the 'elementary species' of such a form as *Erophila vulgaris* (*Draba verna*) may be supposed to have originated. The differences between these are small but constant, and they must be regarded as true species. A variety, on this view, is a relatively transitory form which may at any time be reabsorbed by crossing into the general stock of the species.

We cannot in the present state of knowledge reject altogether the possibility of other modes of formation of new species. Geneticists differ as to the occurrence of radical alteration in the nature of a gene, or of new genes arising *de novo* in the genotype, *i.e.* as to the occurrence of 'mutations' in the narrowest sense, while the interaction of conjugating chromosomes by 'crossing over' is well recognised. We cannot, I think, exclude the *possibility* of long-continued action of the environment actually altering genes or even creating new ones. Thus we have only shifted the problem of variation back. We cannot as yet express variation in terms of chemistry and physics. We do not know what genes are. They may be definite chemical substances, they may be physico-chemical complexes, or some may be of one, some of the other nature. It is certain that a great number must always be present, and that the phenotypic 'characters' must depend on their interaction. We cannot analyse a race of organisms genetically except in respect of those genes that may be present or may be absent. Many genes must be present invariably or the working mechanism would break down—the organism would be non-viable—and these we cannot separate by breeding methods. These things being so, we cannot wholly exclude the hypotheses of orthogenesis and of epharmosis as causes of evolution, much as we may dislike them on account of their vagueness. Modern genetic research has been able to demonstrate to a very large extent the exact correspondence between changes in phenotype and the dropping out and new combinations of genes. But it is impossible at present to demonstrate exactly how such possible processes as orthogenesis or epharmosis may work. We know nothing of orthogenesis except as a phenotypic phenomenon, though we can conceive the possibility that the genotype tends to undergo continuous progressive change in one direction, change which might depend, for instance, on an orderly series of dissociations of molecular complexes, and show itself by corresponding orderly change of the phenotype in one direction. Such a hypothesis would explain certain phyletic phenomena, but we

do not know that it is necessary to explain them thus: they may be brought about in other ways. Epharmosis in the widest sense means simply the continuous adjustment of the organism to its conditions of life. It is often used with reference to external conditions only, but we should not forget that adjustment to external conditions cannot be separated, except by logical abstraction, from the total adjustment of the organism, internal and external. The ontogenesis of each individual is a continuous process of adjustment of every part of the organism to its internal and external environment. So much follows from the universal law that every physical system constantly tends towards equilibrium, and the law is abundantly illustrated in the development of plants. The particular state of relative equilibrium represented by the adult individual is, however, as we know, mainly determined by the stock of genes contained in the zygote from which it is developed, though partly by the particular environment in which it grows up. Epharmosis as a theory of phylogenesis must depend on the belief that the genes themselves can be considerably, continuously, and permanently altered by forces outside themselves—their environment in the wide sense—and it must be admitted that the evidence for such a belief is neither very abundant nor very conclusive. We certainly do not know that genes cannot be so altered; but we cannot point to cases in which it is possible either to assert definitely that they are or to explain plausibly how they may be. On this side the Neo-Darwinian position has not yet, as it seems to me, been successfully attacked, though few biologists who are interested in these questions and not wedded to a particular theory of evolution would now be greatly surprised if it eventually fell.

How, then, are we to make progress to a fuller knowledge of the necessarily interlinked problems of phylogenesis and ontogenesis which together make up the problem of evolution? On the one hand we have the theoretically indispensable genes, of whose nature we have no certain knowledge, though we know a great deal now about the effect on the phenotype of various combinations and omissions of some among them. On the other we have the phenotype, built up from the genes by long and complicated processes of physical and chemical action and interaction between the genes and their derivatives, between the substances and structures of the developing organism, and between these and the environment. Of these ontogenetic processes we still know extraordinarily little. Until quite recently physiology has kept its face averted from such problems, partly as a result of that unfortunate divorce from morphology which we have seen emphasised as a cardinal principle of botanical methodology by distinguished botanists. It must be admitted that these processes are difficult to disentangle, and it is only the great development of physical chemistry, and of the so-called biochemistry which depends so closely upon it, that has opened up during the last twenty years the avenues through which we may approach the problems in this field with any prospect of success. Thirty years ago plant physiologists were mostly either occupying themselves with measuring the 'functions' of the organs of the adult plant under different conditions, or they were caught in the toils of the 'stimulus

and reaction' conception, with its postulate of a series of mysterious mechanisms, supposed to have been built up by natural selection, and apparently inaccessible to further analysis. That this conception was a necessary stage in the development of plant physiology we need not deny; but some physiologists, like some of their morphological colleagues, seem to have rather mistaken a transitory stage of development for an ultimate condition of research. Within the last few years we have begun to get *developmental* physiological studies of all kinds, and some of these are at last beginning to give us some insight into the formative processes which result in the differentiated structures of the plant body. A number of years ago Goebel, in his 'Experimentelle Morphologie,' sketched the connexion between various characteristic external forms of plants and definite factors of the environment. In 1916 one of my predecessors in this chair, Professor Lang, clearly outlined the ideal of 'causal morphology,' and indicated lines on which he thought such investigations should proceed. It is, I think, quite possible to claim that 'causal morphology' in the widest sense *is* morphology proper; to say, with Professor D'Arcy Thompson, that since the problems of form are in the first instance mathematical problems, and the problems of growth are essentially physical problems, 'the morphologist is *ipso facto* a student of physical science.'[17] More recently, again, Professor Priestley and his collaborators have attacked with considerable initial success the question of the actual sequence of events leading to the differentiation of various tissues, more particularly endodermis, cork and cuticle, and have perhaps opened the way to a causal ontogenetic understanding of the whole of the tissue systems of the higher plant.[18]

It certainly seems a far cry from a causal knowledge of these ontogenetic processes, common to whole families or large groups of plants, to an understanding of the way in which the genes which determine the difference of phenotype between one species and another, or one pure line and another, bring about the development of the corresponding phenotype. Superficially at least the kind of character whose origin in the ontogeny Priestley and his fellow-workers have been investigating seems to differ in nature from the kind of character which commonly separates species and varieties. The one is built into the constitution, and helps to determine the economy not only of one species but of a wide range of related species or of great groups of plants; the other, so far as the vital economy of the plant is concerned, often seems to be of no importance at all. To use a metaphor which is perhaps just permissible, the difference is like the difference between the plumbing of a house and the decoration of its façade, or between the lay-out and

[17] D'Arcy Thompson, *On Growth and Form*, 1917, p. 8.

[18] I am aware that there are some physiologists who think that this line of attack is overbold, that our existing knowledge of biochemistry and physiology does not justify a direct attempt to grapple with such problems. I can only say that I am not in agreement with this criticism. The results reached seem to me already to justify the methods employed, though, of course, it may well be that some of Professor Priestley's first conclusions will have to be revised in the light of future knowledge.

construction of its rooms and passages and the lighting of these by a few large windows or by many small ones, where the illumination required is equally well secured by either arrangement. I cannot here undertake a discussion of the justification for separating the 'characters' of organisms into different categories, as Professor Gates, for instance, has tried to do,[19] nor of the related controversy between those who believe that a 'particulate' theory of inheritance such as that which has been worked out by the Mendelians is a sufficient basis for explaining all the phenomena, and those who advocate the claims of the organism to be considered 'as a whole,' which usually means in this connexion cytoplasmic inheritance, through the egg and perhaps sometimes also through the pollen. We cannot wholly exclude the possibility of cytoplasmic inheritance, or an eventual effect on the genotype of cytogenetic characters; but from the broad position I am now taking I see no good reason for supposing that the ontogenetic development of what, for want of a better word, I may call 'organisatory' characters differs essentially from that of the characters which are commonly used to separate species and which obey the Mendelian laws. If we define a gene as some substance contained in the zygote which is a factor in the determination of the phenotype, we must believe that all hereditary phenotypic characters alike, internal or external, separating species or common to a great many species, important, indifferent, or disadvantageous in the life economy, are developed from the genotype, *i.e.* from the total stock of genes, whether contained in the chromosomes or not, by an inevitable series of chemical and physical processes, modified, of course, by differences of environment. Now my point is this. We can only hope to connect the genotype with the phenotype by tracing out these processes in detail, by following the ontogenetic history, not only in terms of the production of organs and tissues, of cell division and growth, but in terms of physical and chemical changes, of such processes as pressures and filtrations, oxidations and reductions, hydrolyses and condensations, reversible reactions and catalyses. And I think we may perhaps begin to find a way which will ultimately lead to an understanding of how the genes produce the characters of the organism, and thus of the nature of the genes themselves, by following the trail which has recently been opened, by studying the detailed processes which lead up to the appearance of a structure, over and above, or, as one should perhaps more fittingly say, 'under and below,' that reaction of structure upon process which we have been used to call the 'function' of the structure. It is only in this way, as I believe, that we are likely, for instance, eventually to get more light on the problem of ontogenetic recapitulation, which has certainly not been rendered easier by the Mendelian results and the conception of the 'species cell.'

The botanists of seventy years ago, notably that great pioneer Sachs, in the spacious days of the new 'wissenschaftliche Botanik' in the 'fifties and 'sixties of the last century, had in some ways a view of the problems of structure clearer than that of their immediate successors. It is plain that the overwhelming effect of the theory of descent on the

[19] R. R. Gates, 'Mutations and Evolution,' *New Phyt.* 19, pp. 217 *et seq.*, 1920.

imagination of biologists, the first brilliant results of the evolutionary interpretation of the doctrine of homology, led to an interest in structure for its own sake which could have but a limited fertility. This interest has in the long run been mainly important because it has immensely increased our actual knowledge of structure. At the same time the very human but really quite irrational desire to find a 'use' for everything led to a facile and sweeping application of the theory of natural selection quite out of accord with the patent facts of nature. The physiologists, the people who really remained interested in tracing causal sequences, in finding out 'how things work,' and who retained the only sound method of discovering this—the experimental method—were rather cut off from the interpretation of structure by the assumption that it was causally 'explained' if it were shown or even plausibly believed to be useful to the organism, and tended to confine themselves to measuring and determining the conditions of processes, mainly in the adult plant. Thus there came about that separation of morphology from physiology which was no doubt a sound methodological principle for the restricted purpose of increasing our knowledge of certain series of facts, but which in its general effect on botany has, I fear, tended not only to disruption but to sterilisation. The effect of the divorce between morphology and physiology was just as bad for physiology as it was for morphology. As little accustomed as the morphologist himself to envisaging the plant in its entirety as a continuously developing complex of substances and structures, the average physiologist tended to limit himself, as has been said, to the recording and measuring under different conditions of arbitrarily selected functions or processes, with the result that his work was often at least as arid as the conventional descriptions and correlations of the morphologist. Needless to say, there were honourable exceptions in both camps.

It is instructive in this connexion to consider a work which professed to deal with tissue structure in the light of function or process—a book thoroughly characteristic of the period I have been considering, the first edition being published in 1884 and the latest (the fifth) in 1918—I mean Haberlandt's 'Physiologische Pflanzenanatomie.' This book describes and discusses each of the tissue systems of the higher plant from the point of view of the part which it plays in carrying on the life functions of the plant as a whole, an excellent aim, and one which is, in the main, admirably carried out. The author makes a great point of adducing experimental evidence for the 'functions' of particular tissues wherever possible. But there is always the implicit assumption that every tissue must *have* a 'function,' must be of some 'use' to the plant, and in his effort to find that use Haberlandt is often compelled to rely on unconvincing argument from structure or from analogy, sometimes on little more than guesswork. It scarcely seems to occur to him that a tissue may have no specific 'use' at all, that structures are developed as the result of the processes which take place in the developing plant, and do not necessarily perform a definite function which is useful to the whole organism. Many of them do, of course; but to confine oneself to the search for such 'functions' is

not the right way to get a real understanding of the structure of a plant. At last year's meeting of Section K the President, Professor Dixon, showed reason to believe that the sieve tubes of the phloem are in the cases which he considered quite inadequate for the purpose of carrying organic substances such as sugars from the leaves to the regions where they are used or stored, as, for instance, potato tubers. What, then, we may ask, is the 'function' of sieve tubes? It seems to me that we should not close our minds to the possibility that they may *have* no 'function' in this sense, that cells having the characters of what we call sieve tubes may quite conceivably be formed simply as the result of the processes going on in certain tracts of developing tissue, without subsequently playing any essential part in the economy of the plant.

The analogy of the machine made by man, in which each part is constructed with a definite object, may be very misleading if we allow ourselves to forget that an organism is not constructed in that way at all, but is the outcome of blind, inevitable processes, and may produce parts which are useless or even harmful to it, provided that the whole is still able to 'carry on' and reproduce itself in its actual conditions of life. We should always approach structure through development, the mechanics, physics, and chemistry of growth and differentiation. It is only thus that we can ever hope to 'explain' structure in any real sense. It is only thus, I believe, that we can ever hope to get back to the real nature of the genes.

The 'functions' of the various organs and tissues—'biological' and 'physiological' functions in the old sense—will then appear in their proper places as those properties or activities which actually contribute to the growth, maintenance, and reproduction of the plant—for the plant must grow, maintain, and reproduce itself, or the race will die. The main essential activities are sufficiently obvious, and we can sometimes say with confidence that if such and such a structure were absent or such and such a process did not take place, these essential activities would be fatally impaired. When a failure of this kind takes place owing to change of genotype or of environment we rarely see it, for it brings extinction in its train.[20] For the most part we cannot know that apparently useful characters could not have been dispensed with, or that metabolic processes might not equally well have taken some other course so far as the success of the plant in the struggle for existence is concerned, while in regard to a multitude of characters there is not only no proof but not the smallest reason to suppose that they have now, or ever did have, any 'survival value' at all. Like all structural features, they are simply products of the plant's activity, though they react in turn to a greater or lesser degree on that activity. Differentiation and so-called division of labour are the inevitable result of increase in size, and of the ensuing different relations of parts of the body to one another and to the surrounding medium. Every type of plant, whether it differs from its parents or not, does and must

[20] In his 'lethal factors' the Mendelian geneticist has, however, succeeded in discovering definite heritable entities which lead to such failure and thus to death. The real nature of these may be eventually ascertainable along the line of research indicated above.

'adapt itself' during its development to its conditions of life. That is to say, it does and must react to the forces, external and internal, acting upon its several parts, and the result of the reaction must be to bring it into closer equilibrium with the whole of those forces. It is sometimes forgotten that 'adaptation' in this sense is a wide physical conception which does not imply that the whole of the characters of an organism are 'useful' to it in the sense in which all the parts of a man-made machine are useful.

Thus we conclude that the central and vital part of botany as a science is, and must be, the study of process which creates and modifies structure as well as of process which is in its turn determined by structure. In reality no line can be drawn between processes of these two kinds, for the development and metabolism of the plant form a continuous connected history in which process and structure continually act and interact. Nevertheless, the 'physiological functions' of adult structures certainly have a special position in that the processes of which they consist are, like the adult structures themselves, the current *terms* of ontogenetic development, the current stages of full expression of the given genotype under the given conditions of life.

The separation of morphology and physiology no doubt ultimately takes origin from the two distinct types of human interest in living organisms, characteristic of different types of mind, the one attracted by the forms, formal relationships and classification of *objects*, the other by the understanding of *process*, the knowledge of working. The one naturally observes and classifies, the other observes and experiments. This kind of separation, clearly enough seen among the older naturalists, has been greatly enhanced on the one hand by the enthusiastic effort to trace phylogeny consequent on the acceptance of the doctrine of descent, on the other by the continuous complication of the physical and chemical knowledge and technique required by the study of physiological processes. It has had a profound effect on the teaching of botany during the past forty years. Botanists whose personal research lay in the one field have been less and less able to take an intelligent interest in the other, even if they could understand the terms in which the results were expressed. The student has perforce come to regard and to study the two fields as wholly distinct, with very few points of contact, and his attention has been directed primarily to morphology largely because it is so much easier for the beginner to examine and cut sections of plants and draw pictures of them than to study the processes which go to the making of them. Too little serious effort has been made to overcome the difficulties of teaching students to study process. The physiologists themselves have been too much absorbed in their apparatus to consider the bearing of their subject on general botany. In recent years the rise of new branches of study, such as cytology, genetics, and ecology, has added to the distraction of the student.

The result has been to separate botany into disconnected parts and failure to give the student any unified notion of the subject. It is unnecessary to say that the growth of knowledge inevitably brings in its train ever-increasing specialisation in *research*, but that fact in no

way absolves the teacher who is responsible for the introduction of students to the subject from the duty of displaying it as a whole, and this he can only do by making its most vital part, the study of process, the key to his exposition, by representing all structure as the result of process, and, in its turn, as limiting and directing process, rather than by concentrating the student's interest on structure and the comparison of structure for its own sake. It seems to me most misleading to represent morphology (in the sense in which it has come to be used) and physiology as if they were equivalent branches of the subject between which the attention of students should be divided. It is only the most superficial view that can regard them as equivalent. Structures are the end results of processes, and to understand them we must study process by observation and experiment. It is unnecessary to remark that thorough and accurate acquaintance with facts of structure is incidentally essential. But to claim the larger portion of the student's time and energy for the work of becoming acquainted with the details of structure of all the various groups of plants involves, in my view, a very serious misdirection of effort.

There should be no division of elementary botany into morphology and physiology. In advanced work there must, of course, be differentiation, as there must in research, not into morphology and physiology, but into a great number of groups of connected phenomena, because of the vast number and complication of the phenomena of the plant world. Some minds find their satisfaction in studying structure for its own sake, so to speak, and in comparing the structures studied. Their research will naturally lie in that direction, and it is certain to increase, as it has in the recent past already vastly increased, our knowledge of the detailed facts of structure of the plant kingdom, to reveal unsuspected relationships, and to establish probabilities as to the lines evolution has followed. But this knowledge *in itself*, considered in relation to the science as a whole, is, and must necessarily remain, superficial. Its conclusions even in regard to the lines which evolution has followed can at the best never attain to more than a considerable degree of probability. And its methods and aims can never explain structure in any real sense. For that a study of process is essential.

The great development in morphological knowledge, especially of what I have called the middle grades of the plant kingdom, and of the great groups of fossil plants which belong to these grades, has, as we must all recognise, immensely increased our acquaintance with the structure of the plant world. It was a natural development of interest in the past history of plants, stimulated and directed by the acceptance of the doctrine of evolution. Looking back upon the history of botany during the past half-century we must be grateful to this movement, and proud of the leading and distinguished part our countrymen have played in its development. But I cannot think that it has had a wholly good influence on the progress of botany, particularly on botanical teaching and research in this country. This has remained too long dominated by the ideal of tracing phylogeny, has given far too much time to the detailed morphology of the different groups which make up

the plant kingdom, and has correspondingly neglected the newer knowledge of process which must be the main avenue to a deeper understanding of plants. Fortunately there are now many signs of impending change. Meanwhile the younger workers, dissatisfied, especially during the last two decades, with the older outlook, have turned more and more to specialised physiological research, to mycology or to genetics, with their outlets on practical life, but often without the grounding that only a thorough grasp of the essentials of the subject can give. One of the results has been that botany has to a large extent become disintegrated, workers in particular parts of the subject having little understanding and less interest in the results of their fellow-workers in other parts. It may be said that this is an inevitable result of the complication of the subject, and no doubt that is partly true. There is a type of professional worker who, having once got immersed in a particular line of research, resolutely refuses ever to come out of his groove and take a broader view. The subject no doubt owes a great deal of its energetic detailed development to such workers. But if botany, as the science of plants, is to retain any meaning as a whole, somebody must retain the power of looking at it as a whole. And if, as teachers, we fail to keep touch with the newer developments, and are consequently no longer able to focus the whole subject from a viewpoint determined by current knowledge, this power will come to be possessed by fewer and fewer botanists, and the subject will definitely and finally break up into a number of specialised and unco-ordinated pursuits.

Do we want that to happen? I think that most botanists would answer 'No!' I do not think there can be any question that the most advanced research worker, as well as the student who never goes on to research, benefits substantially by having had a training which is at once the broadest and the most vital that is possible. As science continuously advances and necessarily specialises, the unexplored fields which lie between the traditional lines of research become of more and more relative importance. They cannot receive adequate attention—the student can, indeed, hardly become aware of their existence—unless his introduction to the subject is continuously informed by the widest outlook and the clearest apprehension of the essential relations of the phenomena of plant life.

SECTION L.—EDUCATIONAL SCIENCE.

THE EDUCATION OF THE PEOPLE.

ADDRESS BY

T. PERCY NUNN, M.A., D.Sc.,

PRESIDENT OF THE SECTION.

In consonance with the general aim of the British Association, the special purpose of our Section is the advancement of educational science. The Section owes its existence to a group of persons who saw clearly that in education, as in all the great fields of practice, there are, and must constantly arise, problems that can be solved only by patient application of the methods of science. The range and importance of these problems were illustrated by Sir Robert Blair in his Presidential Address to the Cardiff Meeting, but I do not propose working over any of the ground which my distinguished predecessor then surveyed. My intention is to take advantage of the customary right of a President to travel outside the strict bounds of his science and to deal with questions which the results of inquiry within its limits illuminate but do not themselves answer.

To a President of Section L the temptation to use this wider liberty must always be strong; for, however far the scope of educational science may extend, the critical educational issues will always lie beyond it. If the term 'education' is used, as it sometimes is, to include all the influences which affect mind and character, it is obviously much more than an applied science. But so it is if the term is restricted, as I shall restrict it, to those formative influences which are brought to bear with some degree of purpose upon the minds of the young. In its origin education is a biological process found not only in all human societies, however primitive, but even in a rudimentary form among the higher animals. By calling it biological I mean that it is a native, not an acquired expression of the race's life, correlative to the race's needs; that it does not wait for deliberation to call it into existence or for science to guide it, but has the inevitability of behaviour rooted in instinct. Thus, as I have argued elsewhere, educational science stands to education in much the same relation as hygiene stands to the physical life; it is a critic rather than an originator; it scrutinises and pronounces judgment upon ways and means, but does not and cannot prescribe the general direction which the educational process shall take. At most it can only help to stabilise the movement by lifting it from the level of instinctive impulse or vague opinion to the plane of ends clearly envisaged and consistently pursued.

What is it, then, that determines the general character of the educational process at a given point in the history of a human society?

The answer is, briefly, that the same *élan vital* which brought the society to that point urges it so to train its young that they may maintain its tradition and ways of life. But this statement needs an important qualification. The consensus of a society never approves of all that goes on within its borders, and among the activities it treats as admissible sets a higher value upon some than upon others. Accordingly the biological impulse which is the mainspring of education tends to select for the training of the young those activities which society judges, consciously or instinctively, to be of most worth. It follows that the education a nation gives its children is, perhaps, the clearest expression of its *ethos* and the best epitome of its scheme of life. Thus the ideas of too many of our Georgian forefathers upon the education of the masses corresponded faithfully with their belief in the great principle of subordination about which Johnson and Boswell talked so often and agreed so satisfactorily. One remembers, for instance, how hotly Miss Hannah More denied the scandalous rumour that she was teaching the poor of Cheddar to write! Similarly, the liberal curriculum of our elementary schools reflects the prevalence to-day of a widely different view of the nature and purpose of society. One is tempted to add that the misgivings with which that curriculum is, here and there, still regarded may be largely due to the ideas of the eighteenth century dying hard in the twentieth.

If what children are taught is but an expression of the general mind of their time and nation, what guarantee is there that education shall be an instrument of social progress and not of retrogression? It must be acknowledged that there is no such guarantee. Among the ideas and ideals, the modes of feeling and action current in a society, it is possible for the general mind to approve the worse rather than the better, and so to give a fatally wrong turn to the training and outlook of whole generations. Have not some of the great tragedies of history thus come about? Such disasters are, in fact, avoided only where the predominant mind of a people has a sufficient sense of the things that belong to its peace. It follows that the ideal 'educational authority' would be neither the teacher with forty years' experience nor the brilliant exponent of educational science, but the *phronimos*—the perfectly wise man who had grasped fully the meaning of man's existence, could see to the bottom of his people's life, appraise justly all its movements, and discern with sure eye its needs. Assuming that he could also communicate his vision to his fellow-citizens, we should do as well under his guidance as the imperfections of humanity would allow.

Unhappily the true *phronimos* appears but rarely, and when he comes bears no unchallengeable certificate of authenticity. If he is not at hand or is unrecognised, we ordinary men and women must apply to our problems the best insight we can attain, trusting that in the conflict of sincere opinions the soundest will in the end prevail. For example, I have referred to the great change in the conception of popular education which has taken place in our time, and have connected it with the steadily growing belief, first, that every member of society has an equal title to the privileges of citizenship; and, secondly,

that the corporate strength of society should be exerted to secure for him actual as well as theoretical possession of his title. How the movement based upon that belief will ultimately affect the happiness of our people no one can with certainty foresee; nevertheless, if one is interested in the wider educational issues one must define one's own attitude towards it. I am, therefore, bound to record my opinion that in its main tendency it ought wholeheartedly to be accepted. I think this chiefly because it seems to be inspired by the Christian principle of the immense value of the individual life, or, if you prefer to put it so, by the Kantian principle that no man ought to be treated merely as a means but always also as an end in himself. But if the movement is accepted, public education must correspondingly assume a character which would follow neither from the principle of subordination nor from the principle of *laissez faire*. The view I submit is that the education of the people should aim at enabling every man to realise the greatest fullness of life of which he is by nature capable—' fullness ' being, I add, measured in terms of quality rather than of quantity, by perfection of form rather than by amount of content. That view is the basis of all I have to say.

Having adopted it, I am compelled at once to face the question, What are the essential qualities of a full life? It is just here that the judgment of the *phronimos* would be invaluable. In his absence I must hazard the conjecture that he would approve of at least the general drift of the following observations. During the last century we learnt, following Darwin, to look upon all biological phenomena as incidents in a perpetual struggle wherein the prizes to be won or lost were the survival of the individual and the continuance of his species. From this point of view there could be only one object of life, one *causa vivendi*, namely, to continue living, and the means by which it was to be attained were adaptations to environment achieved by an individual, and perhaps handed on to its offspring, fortunate germinal variations, or lucky throws of the Mendelian dice. It was natural, if not logically necessary, that the doctrine should fuse with the view, as old as Descartes, that life is but an intricate complex of physico-chemical reactions. Upon that view, even to speak of a struggle for existence, is to use a metaphor admissible only on account of its picturesque vigour; when we study the forms, processes, and evolution of living beings we are spectators merely of the operation of physical and chemical laws in peculiar forms of matter. Thus the occurrence and the phenomena of life are finally and wholly to be explained in terms of the statistical distribution of positive nuclei and their satellite electrons.

These ideas, in either their more moderate or their more drastic form, affected the attitude of men towards matters lying far outside the special province of biology. National policies have been powerfully influenced by them, and it has been widely held that the education of children should be shaped mainly if not solely with a view to ' efficiency ' in the struggle for existence. It is, therefore, relevant to point out what tremendous difficulties are involved in their thoroughgoing application. I will not speak of those which have driven

physiologists of high standing to reject the mechanistic theory of life as unworkable, for I am not competent to discuss them, and they do not bear directly upon my argument. It will be both simpler and more to our purpose to raise, as William James did in the last chapter of his great treatise on psychology, the question of the higher æsthetic, moral and intellectual qualities and achievements of man, and to ask how these are to be brought under the conceptions before us. To be fair we will not press the question how the emergence, say, of Beethoven's Fifth Symphony is to be explained in terms of physics and chemistry; for even the most stalwart mechanists hardly expect that it will actually be done; they only believe that conceivably it could be done. But it is both fair and necessary to ask how the things of which the symphony is typical can be accounted for on the principle of survival-value. James, facing this question with characteristic candour, felt bound to admit that they have 'no zoological utility.' He concluded, therefore, that the powers and sensibilities which make them possible must be accidents—that is, collateral consequences of a brain-structure evolved with reference not to them but only to the struggle for material existence. The premises granted, I do not see how the conclusion can be avoided; but surely it is extremely unacceptable. If, with Herbert Spencer, we could regard art merely as something wherewith to fill agreeably a leisure hour, we might be satisfied by the hypothesis that our sensibility to beauty in form, in colour and in sound, is an 'epiphenomenon' having no significance in relation to the real business of life. But when we think of men whose art was in truth their life, and consider how eagerly the better part of mankind cherishes their memory and their works, it is next to impossible to be satisfied with that view. Or take the case of science. Votaries of pure science often seek to justify their ways to the outer world by the argument that discoveries which seemed at first to have only theoretical interest have often disclosed immense practical utility. It is a sound enough argument to use to silence the Philistine, but would the pursuit of science lose any whit of its dignity and intrinsic value if it were untrue? For instance, would any member of this Association refuse his reverence to the great work of Albert Einstein even if it were certain that, in the words of the famous toast, it would never do anybody any good? I will not lengthen the argument by extending it to the saints and the philosophers, for its point should be already sufficiently plain. The activities of 'our higher æsthetic, intellectual and moral life' have such intrinsic worth and importance that to regard their emergence as accidental and biologically meaningless is outrageously paradoxical. They must be at least of equal significance with anything else in man's life, and may not unreasonably be held to contain the clue to life's whole meaning.

It may be helpful to put the conclusion in other language. Man's life is a tissue of activities of which many are plainly *conservative* in nature. By this I mean that their function is directly or indirectly to maintain the existence of the race and the individual. Agriculture, industry, defence, medicine, are obvious instances of the type, and the list could easily be extended. But there are other activities—I have taken art and pure science as capital instances—whose character, in

contrast with the former, is best indicated by the term *creative*. The point I have tried to make is that in any sane view of human life as a whole the creative must be regarded as at least as significant and important as the conservative activities.

Having travelled so far one must perforce go farther. Purely conservative and purely creative activities, if indeed they exist, are only limiting instances; in most, if not in all activities, the two characters are interfused. For example, the motive of pure science is unmistakably creative, yet its extrinsic conservative value is unlimited; on the other hand, the vast industrial organisations of to-day exemplify activities which, though conservative in their genesis, yet have developed the creative character in an impressive degree. Considerations of this kind prepare one to see that the higher creative life, far from being merely a splendid accident, is really the clearest and purest expression of the essential character of life at all its levels. The poets are, as the Greeks called them, the supreme *makers*, for all making has in it something of the stuff of poetry. In short, there is no life, however humdrum, however crabbed by routine, which is not permeated by the self-same element whose inflorescence is literature, art, science, philosophy, religion.

The argument might rest here, but I am constrained to carry it still farther. I find it difficult to believe that what is true of human life in its conscious aspect is not in some sense true of life as a whole. Competent observers, for instance Professor Garstang, hold that in the animal world there is something strictly comparable with æsthetic creation, but I have in view an idea of wider scope. It is the idea developed with whimsical seriousness by Samuel Butler, namely, that the variations or mutations which in one form or another every theory of evolution postulates, are in essence acts of creation homologous with human inventions and works of art—that if, for example, we compare the emergence or modification of an animal organ, say, with the creation of *Hamlet* or the invention of the petrol-engine, the differences between the two things, vast as they may be, have yet less significance than the fundamental resemblances. This view, which is implicit in some of the older philosophies, is central in the speculations of M. Bergson; it is congruent with the ideas of several modern thinkers who are hardly to be called Bergsonians; and I think it is beginning to invade orthodox biology. It is certainly incompatible with the mechanistic theory of life, but nevertheless leaves room for all that the upholders of the theory are entitled, and (I venture to think) are really concerned to claim. That the life of an organism can be analysed exhaustively into physical and chemical factors is a proposition which it would be extremely rash to dispute; but it is, I think, plainly untrue that the behaviour of the organism as an integrated unit remains within the categories of physical science. Here I take my stand with Professor Alexander and Professor Lloyd Morgan, holding that life is not the mere sum of the physico-chemical reactions that occur in an organism but a constitutive quality of the complex of those reactions—a quality not 'epiphenomenal,' but substantial in the sense that it makes a difference to what Professor Stout has called the executive

order of the world. In Dr. Lloyd Morgan's happy phraseology, the behaviour of an organism *involves* chemical and physical factors, but *depends* on the 'emergent' quality which may properly be distinguished as life. If that be the case, life may well exhibit throughout its range the creativeness which, I have suggested, is one of its essential characters. My educational argument does not stand or fall in accordance with the truth or the falsity of this view; but if the view were well founded the significance of the creative element in human life would be made clear beyond dispute, and the general force of the argument would be greatly strengthened.

The foregoing discussion has wandered some distance from the class-room. Nevertheless it has, I think, a close bearing upon the questions what ought to be taught and in what spirit the teaching should be given. The curriculum, we have seen, always *will* be a partial reflection of the actual life and traditions of a community, and *ought* to reflect all the elements therein which have the greatest and most permanent value and significance. Without doubt these will, in general, be the things that have the highest significance and value for the human family as a whole, but there can hardly be said to be a common human tradition. There exists, it is true, a common European tradition based mainly upon the Græco-Roman and Christianity, and it is vastly important for the happiness of the world to deepen and vivify men's consciousness of it. But even this lacks the concreteness needed to form the basis of popular education—as is seen by contemplation of France and England, two nations that have grown up in it and have influenced one another strongly for centuries, and yet have perfectly distinctive cultures. In short, a nation is the largest social unit whose *ethos* has the necessary individuality. Hence, though we should aim at making our young people 'good Europeans,' we can do so only by shaping them into that particular brand of good Europeans who are rightly to be called good Englishmen. Their education should be, in Professor Campagnac's illuminating phrase, a 'conversation with the world,' but the conversation must, in the main, be conducted in the native idiom. Hence the importance of fostering in our elementary schools the special traits of the English character at its best; of giving English letters a chief place among the studies of our youth; of cherishing the English traditions in the arts and crafts, including our once proud art of music; even (as Mr. Cecil Sharp rightly urges) of reviving the old dances which were so gracious and typical an expression of our native gaiety and manners. Lest this contention should be misunderstood I add that I preach neither the hateful doctrine that what is foreign should, as such, be excluded, nor the ignorant and presumptuous doctrine that what is our own is necessarily the best, and that we have nothing to learn from other peoples. The whole burden of my argument is that the things which have universal human value are the things of most importance in education. But the universal can be apprehended only where it lives in concrete embodiments. In the cases we are concerned with, these are elements or organs of a national culture; and the only national culture to which a child has direct and intimate access is his own. He should be taught to see, as opportunity

permits, how much of it is derived from the common European tradition and how much it owes to the influences of other national cultures; but it should, in its concrete individuality, be the basis of his education.

Lastly, I have urged that among the strains or currents in a national tradition the highest value belongs to those that are richest in the creative element. These are themselves traditions of activity, practical, intellectual, æsthetic, moral, with a high degree of individuality and continuity, and they mark out the main lines in the development of the human spirit. Consider what man has made of poetry and what poetry has made of him; what a noble world he has created out of the sounds of vibrating reeds, strings, and brass; think of the expansion of soul he has gained through architecture and the arts of which it is the mother and queen; of the achievements of his thought, disciplined into the methods of mathematics, the sciences and philosophy. Do we not rightly measure the quality of a civilisation by its activities in such directions as these? And if so, must not such activities be typically represented in every education which offers the means to anything that can properly be called fullness of life?

If the force of the argument be admitted, the principles of the curriculum, about which so much has been written, take a clear and simple shape. A school is a place where a child, with his endowment of sensibilities and powers, comes to be moulded by the traditions that have played the chief part in the evolution of the human spirit and have the greatest significance in the life of to-day. Here is the touchstone by which the claims of a subject for a place in the time-table can be infallibly tested. Does it represent one of the great movements of the human spirit, one of the major forms into which the creative impulses of man have been shaped and disciplined? If it does, then its admission cannot be contested. If it does not, it must be set aside; it may usefully be included in some special course of technical instruction, but is not qualified to be an element in the education of the people.

The same criterion may be applied to the methods by which the subjects of the curriculum are taught. We are constantly told that the 'educational value' of a subject lies in the mental discipline it affords, and, from this point of view, a distinction is made between its educational value and its import as an activity in the greater world; thus geometry is taught as a training in logic, the use of tools as 'hand and eye training,' and so forth. From the standpoint I ask you to adopt that distinction is unjustifiable and may be dangerously misleading; it has, I fear, often been a source of aridity and unfruitfulness in school teaching. The mistake consists in supposing that the disciplinary value can be separated from the concrete historical character of the subject as a stream of cultural tradition. The discipline of the school workshop consists in using the tools of the craftsman for purposes cognate with his and inspired by his achievements. It is because this has not always been done that methods of 'manual training' have too often falsified the expectations of their advocates. Similarly the discipline of school geometry consists not in mastering an abstract scheme or formula of argumentation, but in steeping one's mind in a certain noble tradition of intellectual activity and in gradually acquiring the interests, mental

habits and outlook that belong to it. To say this is not to minimise the importance of discipline or to expel from school studies the austerity which the grave old word suggests. How, for instances, could it be said that our school mathematics represented truly the genius of real mathematics if we neglected the element of laborious accuracy and precision of thought which are essential to it? What is insisted on is that the several forms of mental discipline are characters of concrete types of creative activity, practical, æsthetic, intellectual, and that they influence the mind of the learner favourably only in so far as he pursues those activities as adventures of the human spirit, laborious yet joyous and satisfying, and pursues them after the manner of the great masters. In short, true discipline comes simply by trying to do fine things in the fine way.

The foregoing principles, stated in a necessarily brief and crude manner, are open to misconceptions against which it is desirable to protect them. In the first place, it may seem that I am designing the education of the people upon a scale which may be magnificent but is certainly impracticable. Now I recognise the need of following the advice of a wise official friend who bids one always to bear in mind the magnitude of the educational problem—to remember the slum school and the remote village school as well as the happily placed schools of rich and progressive urban authorities. It is easy, no doubt, to form extravagant expectations, and by seeking to do too much to achieve nothing solid at all. But the argument is concerned far less with the standard to which school studies may be pursued than with their proper qualities and the spirit that should inspire them. In particular, it is directed against the attitude expressed recently by a public speaker who asked what good is poetry to a lad who will spend his days in following the plough and spreading manure upon the fields. Against this attitude it urges that a man's education, whatever his economic destiny, should bring him into fruitful contact with the finer elements of the human tradition, those that have been and remain essential to the value and true dignity of civilisation. This ideal does not assume advanced scholarship or gifts beyond those of ordinary mortals; it implies merely that the normal human sensibilities and powers should be directed along the right ways.

But, it may be objected, granted the soundness of the ideal as an ideal, the shortness of school life still makes it impracticable. This is a criticism to be treated with respect. It is true that a study, to be of real value, must be carried far enough and followed long enough to make a definite and lasting impression. It is also true that some studies can hardly produce their proper effects at all until a certain level of maturity has been reached. For example, there is much of vital moment in science which evokes no response in a pupil before the age of adolescence. But what is to be deduced from these admissions? Surely the conclusion, which the public mind is slowly accepting, that so long as children leave school for good at fourteen some of the best fruits of education will be unattainable and the security of the others precarious. It is not merely a question of length of time, but also, and even mainly, of psychological development. The more carefully youth

is studied the more significant for after-life the experience during the years of adolescence is seen to be. Its importance is not a modern discovery; for even the primitive races knew it, and the historic Churches have always taken account of it in their teaching and discipline. But the problems of what has ever been a fateful period have acquired under modern conditions of life a new urgency. Parents and teachers have worried over them, devoted club-workers have wrestled with them, novelists and psychologists have studied them. In connection with the psychologists, mention of Dr. Stanley Hall's monumental work is as inevitable as it is now superfluous; reference should, however, be made to the recent memoir in which Dr. Ernest Jones has freshly illuminated the old idea that the onset of adolescence marks a definite break and recommencement in mental growth. Especially interesting is the parallelism he establishes between the successive phases of childhood and the corresponding phases of youth. But though in a sense the adolescent retravels a psychological route which he has already traversed in childhood, he is, of course, capable of vastly deeper and wider vision and experience. The case for universal education beyond the age of fourteen depends ultimately upon the importance of shaping his new capabilities in conformity with the finer traditions of civilised life. Public opinion, regretting the generous gesture of 1918, has not at the moment accepted the larger view of the mission of education; but as the nation learns to care more for the quality of its common manhood and womanhood and understands more clearly the conditions upon which that quality depends, the forward movement, now unhappily arrested, will certainly be resumed. For that better time we must prepare and build.

There is another objection to which I should think it unseemly to refer if it were not a stumbling-block to so many persons of good will. A liberal public education will, they fear, make people unwilling to do much of the world's work which, though disagreeable, must still be carried on. The common sense of Dr. Johnson gave the proper reply a hundred and fifty years ago. Being asked whether the establishment of a school on his friend Bennet Langton's estate would not tend to make the people less industrious, 'No, sir,' said Johnson, 'while learning to read and write is a distinction, the few who have that distinction may be the less inclined to work; but when everybody learns to read and write it is no longer a distinction. A man who has a laced waistcoat is too fine a man to work; but if everybody had laced waistcoats, we should have people working in laced waistcoats.'

Lastly, complaint may be made that in all this discourse about the finer values nothing has been said about the ordinary utilities, and the ironical may ask whether it is an error to suppose that the education of the people should furnish them with useful knowledge and abilities. Now the test of utility which the plain man applies to education is, in principle, sound and indispensable; it is, in fact, congruent with the biological origin and function of the educational progress. The only point doubtful is whether the test is always based upon a sufficiently broad idea of utility. The only satisfactory definition of the useful is that it contributes definitely and positively to fullness of life. From that point

of view it is useful to teach a ploughboy to love poetry and not useful to teach a public schoolboy to hate Greek. This is not, I remark, an argument against teaching a subject whose disappearance from our education would be an irreparable disaster. It means merely that the literatures of the ancient world, when taught, should be taught in such a way as to contribute positively to the quality of a modern life. But the term 'useful,' according to the definition, certainly includes utility in the narrower sense. The daily work of the world must be kept going, and one of the essential tasks of the schools is to fit the young to carry it on under the immensely complicated conditions of present-day civilisation. There is no incompatibility between this admission and the general line of my argument. The only relevant limitation imposed by the argument is that what is conservative in purpose shall be creative in its method and, being so, shall embody some dignified tradition of practical, æsthetic, or intellectual activity. The condition may be satisfied by a technical education based upon many of the great historic occupations of men and women—for example, upon agriculture, building, engineering, dressmaking—provided that inspiration is sought from the traditions of the industry or craft at their noblest. Anyone who has a wide acquaintance with the schools of the country will know some whose work accords with these high requirements and gives to practically minded boys or girls an education truly liberal in aim—that is, an education which tends to free their minds from bondage to sordid tastes and petty interests and to make them happily at home among large ideas and activities of wide and enduring importance. What these schools have done and are doing should be borne in mind when Article 10 of the Act of 1918 comes again to life or is replaced by legislative provisions of still bolder design. To conceive 'secondary education for all' as meaning 'the grammar school curriculum for all' would be to make a most serious blunder. The only mistake more serious would be to exclude adolescent boys and girls, even of the humblest station, from any essential part of the national inheritance of culture. But this error may be avoided while full account is yet taken of the far-reaching differences in the talents and *ingenium* of individuals and the rich diversity of the valuable currents, intellectual, practical, and æsthetic, in the life of the community, of which any one may be made the basis of a course truly liberal in quality.

The eminent philosopher, Professor Giovanni Gentile, now Minister of Public Instruction in the Italian Government, has in more than one brilliant work—notably in his eloquent lectures on 'The Reform of Education'—expounded views largely congruent with those expressed in this paper. I welcome his agreement not merely because it may be presumed that the principles he upholds are the principles informing his administration, but even more because the philosophical positions from which we start are widely different. Signor Gentile holds, as I do, that the proper aim of education is to shape the activities of the individual spirit in accordance with the best traditions of the human movement. In particular, he does not shrink from insisting that the simplest instruction in the primary schools should be offered in the true spirit of culture. And he also maintains that the education of the

people must be national in its general setting. Indeed, I venture to think that he sometimes carries this idea too far—appearing to advocate as an end in itself what should surely be only the means to a broader end, and to forget his noble declaration that the teacher must always stand for the universal. This is an error hard to be avoided by a philosopher whose inspiration is largely Hegelian; moreover, it is easily pardonable in a patriotic speaker with the glorious cultural history of Italy behind him and before him the elementary school teachers of Trieste *redenta*. But although I regret Signor Gentile's adhesion to what I consider a false view of the relation between the individual soul and society, his book has high value, for it expresses a passionate conviction that during the last century the development of the great European peoples went in some respects sadly astray, and that their moral health can be restored only by education inspired from top to bottom by a true judgment of values. Here he is, I believe, fundamentally right. The last hundred years have greatly accentuated the gravity of a problem which was discerned by the poet Schiller and diagnosed in the famous 'Letters on Æsthetic Education' he published in 1795. To this diagnosis Dr. C. G. Jung has devoted an interesting chapter in his book on 'Psychological Types.' In Schiller's view the immense progress of the modern nations has been purchased at the expense of the development of the individual soul, so that, in spite of the greatness of our achievements, we are, man for man, inferior to the various and well-rounded Athenians of the best days. It is the division of labour essential to a large scale organisation of society which has at once made general progress possible and individual impoverishment inevitable, for it has cut individual men off from experiences that are indispensable to the full well-being of mankind. If this was true in the days of the French Revolution, how much more true it is to-day, and how much more grave the evil. We are told that before the era of industrialism the great mass of our people enjoyed a culture which, though simple, was sincere and at least kept them in touch with the springs of beauty. What truth there is in the picture I do not know, but it is certain that with what is called the industrial revolution the conditions that make it credible largely disappeared. Torn from the traditions of the old rural life and domestic industry and herded into towns where in the fight for mere existence they lost their hold on all that gave grace to the former life, and where the ancient institutions which might have helped them to build up a worthy new one were themselves submerged in the rising tide of featureless and monotonous industrial activity, the folk who now constitute the bulk of our population were cut off effectually from 'sweetness and light.' That was the situation when the task of public education was taken seriously in hand, and that, notwithstanding a great amelioration in details, is for far too many the situation to-day. There are some who think that the only remedy is to cry halt to the modern movement and return deliberately to mediævalism. This is, I fear, a counsel of despair; instead of indulging idle dreams it will be more profitable, assuming the unalterable conditions of modern life, to consider how the rest may so be modified as to place the true dignity and grace of life within the reach of all who are qualified to achieve them. That

can be done only by a system of education which brings the things of enduring and universal worth to the doors of the common people. It is what has been done by many an elementary school teacher, sometimes with scant assistance from public opinion, simply because, face to face with his helpless charges, he was impelled to give them the best he had to give. It will be done with increasing happy results the more clearly it is seen that the proper function of the elementary schools is something much more than to protect the State against the obvious danger of a grossly ignorant populace or to ' educate our masters ' in the rudiments of citizenship. And unless it is done, unless the natural hunger of the people for knowledge and beauty is wisely stimulated and widely satisfied, no material prosperity can in the end save the social body from irretrievable degradation and disaster.

SECTION M.—AGRICULTURE.

SCIENCE AND THE AGRICULTURAL CRISIS.

ADDRESS BY

CHARLES CROWTHER, M.A., PH.D.,

PRESIDENT OF THE SECTION.

IN addressing the Section as President I would confess at the very outset to a pride that I should be permitted to occupy a post of such great honour, for which my chief qualification must be that of having graduated through every other office provided for in the Sectional organisation.

I could only have wished that the honour had fallen to me in any year other than the present, in which my energies have been fully absorbed by the duties of a new appointment of a peculiarly difficult character; and it is with some misgiving that I venture to address the Section to-day, being conscious of having nothing to offer but a few random thoughts, incubated at odd moments, and reduced to verbal form under conditions which have not permitted the careful revision that the occasion demands.

For the second consecutive year the Section meets in a great sea-port, a city whose activities are written large across the history of British agriculture throughout the past century, and have contributed in no small degree to the anxieties with which the industry is beset at the present day. The part played by the port of Liverpool in shaping the fortunes—or misfortunes—of British agriculture might well have formed an appropriate subject for the Presidential Address to this Section, had I possessed the competence and leisure to deal with it effectively, but I must confine myself to matters falling more closely within the range of my everyday activities.

When the Section met last year British agriculture was reeling under the shock of a second disastrous year, which in large sections of the industry, notably those dependent primarily upon the direct sale of crops, seemed likely to produce a crisis of the gravest character, and greatly accentuated the existing anxiety even in sections of the industry less directly affected. This atmosphere of crisis still unfortunately persists, though permeated now perhaps by a rather more optimistic note, and it must necessarily receive the consideration of this Section of an Association which aims at intimate touch with the every-day life of the nation.

It is generally recognised that the primary causes of the present difficulties of British agriculture are strictly economic in character, and not due to any gross and general failure to apply present-day scientific knowledge to the technique of farming, although the great disparity which exists between the average production of the country and that secured by the more competent farmers on soils of the most diverse

natural fertility suggests that with a higher general level of technique and education the intensity of the crisis might have been sensibly reduced. So far, however, from there having been any appreciable lowering in the general standard of our farming, as measured by the application of the teachings of agricultural science, it is the common experience of those of us who are in close touch with the farming community that recent years have witnessed a very marked and rapid development amongst farmers of interest in agricultural education and research. Throughout the more intelligent section of the older farmers and the whole body of the younger men the old antagonism between 'practice' and 'science' is rapidly disappearing. Whether it be a case of the 'sick devil' or not, the agricultural community is at present in a more receptive mood towards scientific advice than at any time I can recall in some twenty years' advisory experience, and I believe the moment to be opportune for a forward movement in agricultural education, which, if wisely developed, may remove the last vestiges of opposition and establish education and research firmly in their rightful places in our agricultural organisation.

I have referred to the causes of the present crisis as being strictly economic, and such palliative measures as have been adopted or suggested have been almost entirely aimed directly at immediate economic relief. There is, indeed, the danger that if the exponents of agricultural science remain silent the impression may get abroad that we have nothing substantial to offer towards the alleviation of the crisis, and it is my main purpose to-day, therefore, to indicate some of the directions in which I believe help can be given, and some of the lines along which development of our scientific and educational organisation is, in my opinion, more especially necessary at the present juncture.

Our agricultural educational system may be likened to a pyramid with research at the apex, elementary education and general advisory work at the base, with intermediate education, higher education, and higher advisory work occupying the intervening parts. Our pyramid has grown within the last thirty years from a very modest structure of low elevation into an imposing edifice, which perhaps appeals to the mind's eye more through its height than its spread, the upward growth having taken place at a proportionately greater rate than the expansion of the base. Such, at least, it appears to me, and I shall suggest to you later that the essential need of the moment is a broadening of the base with a view to greater stability and a more effective transmission of the results of the activities of the upper portions to the maximum basal area over which they can beneficially react.

For the purposes of my survey it will be convenient to follow the customary classification of our work into research, advisory work, and teaching. Of these three divisions I propose to deal but very briefly with the first, that of research, since the potentialities of research for the advancement of agriculture are too patent to require exposition, the ultimate object of all agricultural research being the acquisition of knowledge which will enable the farmer to comprehend his task more fully, and to wield a more intelligent control over the varied factors which govern both crop production and animal production.

Agricultural progress must be dependent upon research, and no phase of our agricultural educational system is so full of great promise for the future as the comprehensive research organisation, covering practically every field of agricultural research, which has been brought into existence during the past twelve years, and developed upon lines which ensure an attractive career to a large number of the most capable research workers coming out of our universities. In praising the Research Institute scheme I am not unmindful of the needs of the independent research worker and the spare-time research work of teaching staffs—the type of research work to which we owe so much in this country—and it is with some anxiety that I have watched the distribution by the Ministry of Agriculture of the modest resources available for the support of this class of work. I trust that my fears are groundless, but I am afraid of a tendency to deflect such resources towards the work of the Research Institutes, a tendency which in common fairness to the independent worker should be most strenuously resisted. With a sufficiently liberal conception of the class of work which can be effectively carried through by the independent worker there should be no difficulty in allocating these moneys to the purposes for which they are intended.

In suggesting, as I did a few moments ago, that in proportion to the means available agricultural research is perhaps more adequately provided for at the moment than other branches of agricultural educational activity, nothing is further from my mind than to imply that greater resources could not be effectively absorbed in this direction, but I am guided by the feeling that a due measure of proportion should be maintained between research and the organisation behind it designed to translate the findings of research into economic practice, and to secure that each advance of knowledge shall be made known quickly and effectively throughout the industry.

It is chiefly in the latter direction that agricultural science can make an immediate and effective contribution to the alleviation of the present crisis, since agricultural research in the main does not lend itself to the 'speeding-up' necessary for quick action. The same applies also to formal educational work, which must necessarily exert its influence on the industry but slowly.

The one line of approach along which agricultural science can make its influence felt quickly is that of *advisory work*, which consists in the skilful application of existing knowledge to the solution of practical problems, or at most the carrying out of investigations of a simple type, with a view to securing guidance as to the solution of the problem in time for effective action to be taken.

It is, therefore, to the possibilities of such advisory work that I propose to turn my attention in more detail. The root difficulty of agricultural educational propaganda in the past has been to secure a sufficiently intimate and widespread contact with the farmer, and for this purpose no agency at our command is so valuable as advisory work, since it ensures a contact with the individual farmer which is both direct and sympathetic, originating, indeed, in most cases out of a direct request for help. The difficulties in the way of extending advisory work

greatly I shall turn to presently, but I wish first of all to outline some of the more immediately helpful forms of advisory work which have fallen within the scope of my own personal experience.

When some four years ago I undertook to develop for the late Lord Manton a research and advisory organisation to furnish guidance in his extensive farming enterprises, I was obliged in the first instance to take account of the fact that the resources at my disposal, though large, would not serve to cover the whole field of agricultural problems, and so far as specialist work was concerned it would be necessary to concentrate on two or three fields of activity, outside which only general guidance could be afforded by the departmental staff and for specialist assistance it would be necessary to have recourse to the national advisory organisation set up by the Ministry of Agriculture. Eventually, after careful consideration, the fields of work selected for special attention were those of soils, plant nutrition, plant breeding, and animal nutrition, and it is to these that I propose to refer more particularly. No specific provision was made at the outset for dealing with diseases, either plant or animal, partly for reasons of economy, but mainly because it was felt that the outstanding disease problems could be more effectively dealt with by co-operative effort through the national organisation than by a small isolated advisory station.

In making provision for soil work as one of our principal lines of activity I was actuated by the conviction that soil investigation is the most fundamental of all forms of agricultural research. Soil factors dominate the growth of crops from germination to maturity, and must influence the utilisation of the crops by the animal, which is their ultimate destiny. In stressing the importance of soil advisory work I am not unmindful of the fact that, despite the enormous volume of investigation relating to soils which has been carried out, the task of the soil adviser still remains a very difficult one, and except in a few directions, and over a comparatively small area of the country, the interpretation of soil analytical data is rarely clear. It is a sobering thought, indeed, to recall the abounding optimism with which soil analysis was entered upon some eighty years ago, and contrast the hopes then held with the realities of soil advisory work as we find them to-day. The initial mistake—so common throughout a large part of our agricultural investigational work of the past—lay in a failure to visualise the complexity of the problem, even with due regard to then existing knowledge. The problem was approached as if the soil were to be regarded solely as a reservoir of plant food, whose capabilities for crop production should therefore admit of complete diagnosis by chemical analysis. The conception is fascinating in its simplicity, and has dominated the greater part of our soil work down to the present time, repeated endeavours being made by variation in the methods and intensity of the analytical attack to improve the persistently low degree of correlation between analytical data and crop results. Parallel with this at a later date was developed the mechanical conception which found the major part of the explanation of the differentiation of fertility in the physical properties of the soil particles, whilst still later soil biology has asserted its claim to provide the 'simple solution.' The work of recent years,

however, so brilliantly led in this country by Sir John Russell and his colleagues, leaves us with no excuse for such restricted conceptions of soil fertility, which must now be regarded as the index of the equilibrium established by the mutual interactions of a highly complex series of factors, the variation of any one of which may affect the interplay of the whole, with consequent effect upon the rate or character of plant growth.

The problem of fertility being so complex, one might perhaps be inclined to despair of attaining to anything really effective in soil advisory work, which must necessarily be dependent upon rapid and somewhat superficial examination, and such apparently is the view held by the Ministry of Agriculture if one may judge by the conspicuous neglect of chemical and physical science in recent extensions of advisory facilities.

My own conception, however, of the present possibilities of soil advisory work is more optimistic, and from experience covering the most diverse parts of the country I am confident that an extension of facilities for soil advisory work would be of immediate and progressively increasing benefit to the farmer.

It is the common experience of all engaged in soil advisory work that, although what may be termed the 'average soil' offers great difficulties, there are many soils in all parts of the country which are distinctly not 'average' for the areas in which they are situate, and for which our conventional methods of chemical and mechanical analysis, crude though they be, and imperfect the premises upon which their interpretation is based, do yield guidance which on application in practice proves to have been substantially sound. The real difficulty at the moment is that for large tracts of the country we lack the necessary data to enable us to determine what is the 'average soil' for each particular area, and until provision is made for specific soil work in these areas, which comprise the whole of the great agricultural areas of the Midlands, our advisory work relating to this raw material of crop production must of necessity remain superficial, and only too frequently ineffective.

In no direction has the need for extended soil advisory work become more evident in recent years than in the revelation of the extent to which large areas of our soils have become depleted of lime. Cases come almost daily to our notice in which this lack of lime is clearly the chemical 'limiting factor,' and the annual waste due to unremunerative expenditure on fertilisers on such land must indeed be very great. In many cases, fortunately, the depletion has been detected at a stage at which it is still economically remediable, but in others, unfortunately, this is no longer the case, and unless soil-survey facilities be greatly extended it is certain that large areas of our land must steadily fall into the latter category, with the inevitable development in the near future of a problem of such magnitude as will require national action for its solution. It is worthy of note also in passing that this problem will probably be accentuated rather than diminished as a greater proportion of our arable land reverts to grass.

A further direction in which great scope remains for the work of

the soil adviser is in the economic manuring of crops. More attention has probably been paid to the subject of manuring than to any other branch of agricultural science, and this branch has been perhaps more definitely systematised than any other; but inadequate and improper manuring is still widely prevalent, and the annual wastage of resources thereby incurred must represent a very large sum. A considerable part of this wastage is due to the widespread use of proprietary compound manures, more often than not compounded without any special reference to the soils upon which they are to be used, or even without intelligent adaptation to the special needs of the crops for which they are supplied. It is not uncommon, indeed, to find mixtures of identical composition offered for the most diverse crops. In far too many cases also the prices charged are extravagantly disproportionate to the intrinsic value of the ingredients of the mixture, and in all these various ways costs of production are made higher than they need be. In claiming that improved manuring achieved through extended advisory guidance might effect a sensible alleviation of the present difficulties of the arable farmer, I am not unmindful of the fact that even the best practice may result in loss when the value of produce sinks to the low levels recently touched by many crops, and the best manuring will not make it possible, for example, to grow potatoes profitably under present conditions for sale at 30*s*. per ton. Where loss is inevitable, however, this will usually be lowest at a level of production involving the reasonable and intelligent use of manures.

Passing on from soil and manuring, we come to the sphere of seed and sowing problems, presenting obviously abundant scope for advisory work. The need for good and pure seed is axiomatic and is recognised by the existence of the Seeds Act, which remains to us as a legacy, more beneficent in its operation than many others, of the war-time interest of the State in agriculture.

Seed must not only be good, however, but it must be of the right kind, sown under proper conditions and at the most suitable time, and the value of advisory guidance on these points has always been recognised, especially with reference to the choice between different varieties of each particular crop. The variety tests carried out on the various college farms and elsewhere have always proved helpful in this respect in so far as they serve to demonstrate the general characteristics of the different varieties. Whether they have been equally successful in measuring the cropping capacities of the different varieties is more than doubtful, owing to their restriction to single, or at most double plots of a kind, and this has been recognised in the more elaborate schemes devised for the purpose by the National Institute of Agricultural Botany, which it is to be hoped may furnish a practical scheme for more accurate quantitative field tests in the future.

Given good seed, the improvement of crop possible through seed selection is perhaps not in general so striking as that frequently obtainable by manuring, but it may nevertheless be substantial, especially with crops such as barley, where improvement of quality may have a special value. There is also a rapidly extending field for seed advisory work in connection with the laying down of land to grass for varying periods.

During the growth of the crop advisory work is largely restricted to the domain of diseases and insect pests, whose ravages take incalculable toll of our crops. This section of advisory work I am not competent to discuss, but I am continually impressed by its importance as I note how largely such matters bulk in the inquiries for assistance which pass through my hands, and I believe science can make no more directly effective contribution towards the removal of at least the technical difficulties of the farmer than the elaboration of effective preventive measures against pests and diseases.

In some directions, as in the circumvention of certain diseases of potatoes and cereals, very striking advances have already been made, to the great benefit of practice; but in all too many cases the adviser at present can go little beyond the stage of diagnosis, although, with the greatly increased number of research workers now available, there are good grounds to hope that the lines of preventive action may before long be worked out.

I must pass on, finally, to the utilisation of crop products as food for animals, the line of work with which my own personal interests and activities have always been most closely associated. Looking back over twenty years of advisory activity, I realise that the position of the adviser in animal nutrition is infinitely stronger to-day than when I first assumed the *rôle*.

At the outset of this period the feeding of animals was regarded simply as a matter of supply of suitable proportions of digestible protein, oils, and carbohydrates, more or less regardless of the character of the materials in which they were supplied. Little further could be done in the way of differentiating the values of different food materials beyond a comparison upon the basis of gross digestible energy, although the conclusions to which this led were notoriously unreliable and in many cases in flagrant conflict with practical experience. Material for a great advance was, however, rapidly accumulating in the work of Kellner, which was finally reduced by him to a practical system of food evaluation in his classic ' Ernährung der landwirtschaftlichen Nutztiere,' published in 1905, and universally acclaimed as representing a great advance in the application of nutritional science to the practical feeding of farm live-stock. The advance lay essentially in the discrimination between the available energy and the net energy of foods, and the carrying out of a sufficiently large number of determinations of the latter to furnish a fairly adequate basis for generalisation. With these to supplement his classic determinations of the values of protein, fat, and carbohydrate for the production of fattening increase, he was able to devise a practical scheme of assessing the production-values or net energy-values of foods, which he preferred for reasons of practical convenience to express in terms of starch. The significance of the great practical advance made by Kellner was not at first clearly grasped in this country, critical attention being directed, in accordance with true British conservatism, more to the admitted shortcomings of the starch-equivalent than to its merits; but as time revealed its superiority over the older methods it came generally into use, and now serves as the basis of all our advisory work in farm nutrition.

Although primarily designed for the case of the fattening animal, it has proved practically useful for other classes of stock, and even, with slight modification, for the case of the milk-producing animal.

The last twenty years has also witnessed the great developments of protein investigation which have thrown much light upon the problems of protein metabolism and the productive efficiency of the proteins of different foods. Lastly, we may recall the remarkable developments in nutritional science of recent years, consequent upon Hopkins' discovery of the 'accessory growth factor,' and also the attention which is now being directed to the importance of the mineral ingredients of foods.

With all this newer knowledge at his command, the adviser in nutrition can now approach his work with far greater confidence, and evidence of the increasing practical value of his work is rapidly accumulating. This is particularly the case with advisory work in milk production, a branch of feeding which lends itself more readily than most to carefully regulated rationing owing to the ease with which the amount of product can be determined. Few branches of advisory work have proved more directly helpful to the farmer in recent years than this advisory control of the feeding of dairy cows, the extension of which has been greatly aided by the development of milk-recording societies, in whose activities such rationing advice is rapidly becoming regarded as an indispensable feature. Much success has also been met with in advisory work in pig-feeding, and to a less extent in the feeding of cattle, the lower degree of success in the latter case being due not so much to an inferior capability of the adviser to help as to the difficulty of dispelling the tradition that beef production represents the supreme accomplishment of the British farmer, as to which there is nothing left for him to learn. The work already accomplished represents, however, but the very beginnings of economy in the feeding of live-stock, and wasteful feeding of both home-grown and purchased feeding-stuffs for lack of the necessary advisory guidance is still far too widely prevalent.

Such are only a few of the aspects of advisory work, which, if extended more widely, might exercise a very profound effect upon the economy of the industry. Such extension implies, however, greatly increased resources in men and money and more efficient means of bringing the advisory facilities to the notice of the farmer.

I am inclined, indeed, to think that a more efficient propaganda is perhaps the first need of the situation, as one finds in all parts of the country an astonishingly large number of farmers who are totally unaware of the existence of advisory facilities of any kind. A more extensive propaganda will be useless, however, unless accompanied by increased provision for advice, since the present resources are already more than fully taxed by the relatively moderate volume of calls for assistance that now arise. It is the universal complaint of the County Agricultural Organisers that they cannot secure the personal contact, which it is the most important part of their functions to establish, with more than a very small fraction of the farmers within their area, and it is for a great extension of this type of advisory assistance that there is the most clamant need. Most of our counties have, at present,

only one agricultural adviser—some, indeed, have none—and yet this slender organisation represents in large measure the base of contact with the industry upon which the whole pyramid of our advisory and educational work rests. It is here where I see the most immediately profitable outlet for any further moneys that may be available for agricultural education in the near future. The facilities for organised instruction in agriculture are at present adequate for the numbers of students coming forward, or likely to come forward, in the near future, the present problem in this sphere being indeed rather that of finding suitable openings for the numbers of students passing through our courses—a matter to which I shall return presently.

I have already alluded to the chemical gaps in our specialised advisory organisation, and I might also have indicated the similar and even less comprehensible inadequacy in the provision for specialist advice in economics; but these are relatively small matters compared with the paucity of the less highly specialised but scientifically trained advisers of the County Organiser type, whose business it should be to secure the confidence of the individual farmer by personal contact, and the rendering of assistance either directly in the simpler problems or with the help of the specialist staff standing behind them in more complex cases, whereby a more widespread and real appreciation of the practical value of agricultural education and research than now prevails might quickly be developed.

A great extension of advisory work such as I suggest, must necessarily involve heavy expenditure, and further, an exceptional measure of care in the selection of men, since in the direct approach to the farmer personal qualities may in the first instance count for more than technical proficiency. Furthermore, if the full measure of success is to be achieved, it is essential that a more closely organised and intimate contact should be established between the various units of the advisory organisation, from the research station through the scientific adviser, to the practical adviser. Our present organisation is too indefinite and too widely permissive in this respect and calls urgently for consideration by all concerned, both county authorities and advisory and research workers, with a view to more effective co-ordination and co-operative effort.

I have laid great stress upon the potentialities of advisory work as a contribution to the alleviation of the present crisis, but I cannot close without some reference to the far greater contribution to the future prosperity of British agriculture which we can make through our educational system, if wisely pursued, in the training of the farmers of the future.

I have already expressed the opinion that the existing facilities for organised agricultural education—at least so far as universities and colleges are concerned—are adequate to deal with the numbers of students presenting themselves. There is indeed at the moment a considerable excess output of the class of student who is either unwilling or unable to take up practical farming and must needs have a salaried post. This problem, which is becoming an increasingly serious one, especially for the non-university institution, such as my own College, hardly falls, however, within the scope of my present theme, except in so far as the

extension of advisory facilities I have advocated would tend to absorb this surplus and restore the balance of the whole organisation.

Of more immediate concern is our comparative failure to secure for our educational courses more than a small fraction of the sons of farmers, upon whom the future of the industry will largely rest. I have testified to the greatly awakened interest in agricultural education which has been displayed amongst farmers in recent years, but it is yet far from having developed into a conviction that such education is to be regarded as a vitally essential part of the farmer's training. One must perhaps be content with gradual advance towards this goal by internal development, although the possibilities of more rapid advance by external pressure should not be overlooked. One such that might have a more potent influence than any other in filling our colleges with farmers' sons I would submit for the consideration of my distinguished predecessor of last year, in supplement of his able exposition of the part to be played by the enlightened landowner in the progress of agriculture. It is that in letting his farms—at any rate so far as young applicants are concerned—the enlightened landowner should show his faith in agricultural education by giving first preference—other considerations being equal—to men who have received adequate instruction in the principles of agriculture in addition to practical experience. So long as the private ownership of land continues—and I trust that it may be very long—the landowner will have it in his power to render the most powerful aid to the progress of agricultural education, and by action along the lines I have suggested might exert more good in one year than is attainable by many weary years of propaganda. Whatever the character of our land tenure system of the future, it is certain that sooner or later some guarantee of efficiency for the productive occupation of land will be demanded from the would-be farmer. We cannot continue indefinitely, on the one hand, to proclaim that the land is our greatest national asset, to be maintained with the help of, and in the interests of, the State in a highly efficient state of productivity, whilst, on the other hand, the use of the land is left open to all, regardless of fitness for its effective use. This vision of farming reduced to the status of medicine and law as a close profession regulated by an entrance examination, may perhaps be stigmatised as a horrible nightmare; but some movement in that direction I believe to be inevitable, and, with nationalisation of the land, might well come more speedily than one would venture to contemplate. None will question, at any rate, that, should such a day arrive, education in the principles underlying the calling will loom as largely as practical training in determining the standards of admission to the use of the land. I will conclude on this highly imaginative note with an expression of my firm conviction that the genius of the British race for the management of its affairs on lines of voluntary action will not desert us in this particular, and that with wise guidance and intelligent adaptation of educational curricula and methods to the changing needs of the times the penetration of our practice by science will proceed smoothly and with such rapidity as to render interference from outside not only unnecessary, but unwarrantable.

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